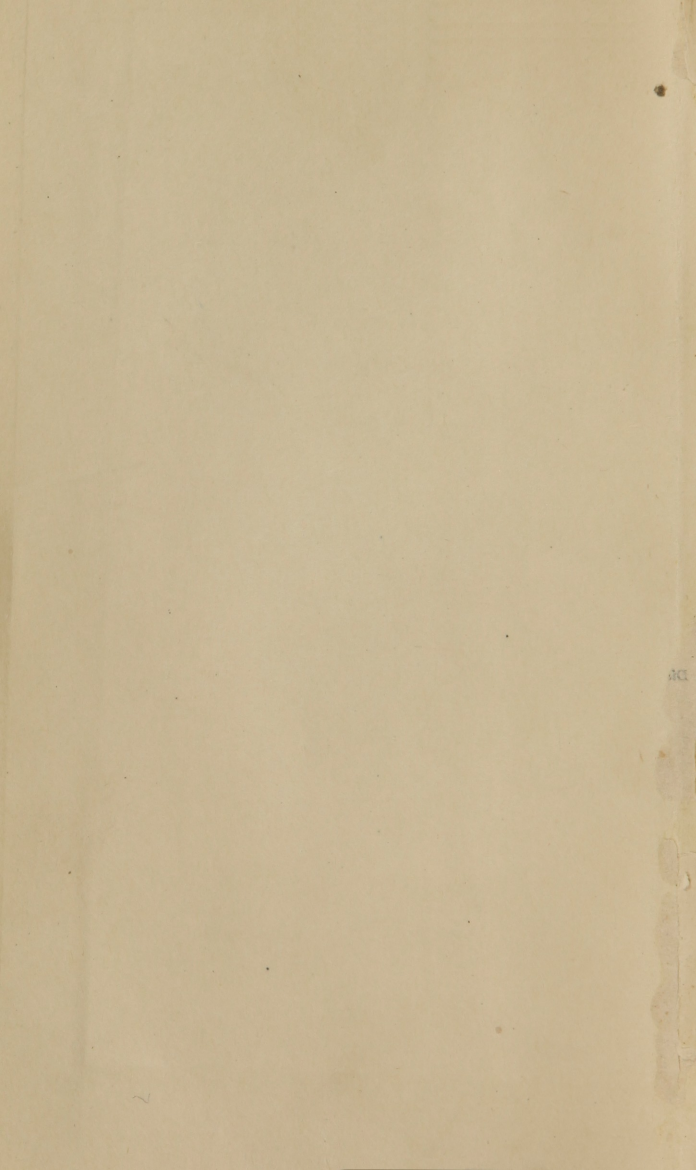


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ON THE
INJURIOUS EFFECTS
OF
MINERAL POISONS
IN THE PRACTICE OF MEDICINE.

COMPRISING AN EPITOME AND COMMENTARY ON
THE SYSTEM OF LUIGI CORNARO FOR ATTAINING OLD AGE,
AND ON
THE NEW SYSTEM OF MEDICINE OF F. V. RASPAIL
(*Every Man his own Physician*),
To which the MONTYON PRIZE of 10,000*l.* was lately adjudged at Paris,
EMBRACING ALL THE DETAILS NECESSARY FOR PERSONS TO GIVE THIS SYSTEM A TRIAL,
&c. &c.

BY
HORATIO PRATER, M.D., PH. D.

*discoverer of the Fusible Compound of Carbon and Silica ; of the peculiar Agency of Lime
on the Tonicity of Muscular Fibre ; of the Permanent Fluidity of the Blood by a
Heat of 140° ; of the Acceleration of its Coagulation by Ammonia, Car-
bonat of Soda, &c. ; of the Cause of the Coagulation of Albumen
by Heat ; of the Diffusive Power of the Gases ; and,
conjointly with Fizeau, of Möser's Images ;
and Author of " Experimental In-
quiries in Chemical Physiology,"
&c. &c. &c.*

"Un uomo non può esser medico perfetto d'altri fuor che di se solo."—CORNARO.

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P R E F A C E.

THE great object of the present little work, consisting of different Essays, written at different times, is to banish the use of mineral poisons *entirely*, and vegetable poisons *almost* entirely, from the Practice of Physic, and if it shall be in the slightest degree instrumental towards accomplishing this object, I shall not consider it written in vain. “*Delenda est Carthago*” should be the sole cry of every one interested in the public health until all the mineral poisons are expunged from our pharmacopœias. It is, indeed, rather a singular coincidence that these were introduced there chiefly by a man whom the medical world now delight to call a Quack—by Bombastes Paracelsus! and I trust I have not chosen an inauspicious time to send this “little book from out my solitude,” now that manganese is putting forth its claims to become legitimised and get inscribed by the side of its metallic brethren in the Pharmacopœia; and perhaps I may also add gold, seeing that cases are collecting fast of the good effects of this in a disease which, if let alone, or *nearly so*, gets well of itself! I allude to syphilis.

While metallic *poisons* are permitted to remain in the Pharmacopœia, no doubt medical examinations cannot be too severe, lest *immediate* death should follow their administration; but why do they remain there? Why make a mystery where there is none, and fill a volume where a few sheets of paper would suffice? The quackery of physic, which Adam Smith properly considered existed also among regular practitioners themselves, consists in the attempt to make a science where there is none, and in the assumption of superior knowledge over the public at large *on a point where nature permits not such superiority*; for where all is chance, or next to it, surely the chances of all to hit on “a remedy” must be tolerably equal. I repeat, gentlemen of the Colleges of Physicians, it is you that make examinations imperative, *not the necessity of the case*. Nature ever works in a simple way, and will generally recover herself by abstinence, rest, a purgative, and change of air; but certainly not so easily after mineral poisons have been used, however skilfully applied. I urge no objections to examinations in surgery, for here we have something ostensible and tangible to deal with, and we can see or ascertain by manual examination the injury or the disorder. But in medicine, numbers of inscrutable causes—such as peculiarity of constitution, temporary peculiarity of condition of the alimentary canal from nervous influence, or from the remains of solids or fluids not wholly assimilated, &c.—constantly oppose our efforts at correct induction, and prevent

the *very same* substances from acting in the same way on two different individuals. In such case, then, how uncertain must be all so-called science !

Surely, gentlemen of the colleges, if any extra argument were necessary to convince you of the nonentity of the science of physic, it would be the fact that you have derived most of your knowledge from empirics,—from Paracelsus formerly, from Priestnitz and, I may add, Raspail in the present day ; for both these *innovators* have made many converts among regularly educated physicians. Singular science, indeed, where empirics come *at last* to be the very teachers of the profession !

I have written plainly, gentlemen, because I believe you have only what you consider the interest of the public at heart in any article inserted in your Pharmacopœia. I, therefore, make this appeal to you to reconsider the matter ; for you must be fully aware of the complete uncertainty of medicine, and that it is just as much a conjectural art (“*ars conjecturalis*”) now, as it was when Celsus wrote and called it so. Under such circumstances, surely that system is the best *which permits itself to do the least harm*. Such a system, then, is that which excludes the use of mineral poisons. What said the celebrated Dr. Frank in regard to *ordinary* medical practice ? “*Thousands are slaughtered in the quiet sick room !*” (Weinholt’s Lectures on Somnambulism, by Colquhoun, p. 18, preface.) It is a distinguished physician himself who here speaks, and so forcibly

brings to mind the passage of Tacitus, “Solitudinem faciunt; pacem appellant.”

Since the discovery of the *stomach-pump*, the use of the mineral emetics—tartarized antimony, sulphats of copper and zinc, &c.—even in cases of poisoning, is far less justifiable than formerly, when their greater strength might have been urged as a reason for employing them. Now with ipecacuanha, warm water, and the stomach-pump, we have all that is necessary, even in the worst cases of poisoning. If we want not then, now, mineral poisons even to cure poisoning, how monstrous to think we want them to cure diseases!

“If things were but called by their right name,” the present system of the College of Physicians might justly be called the *poison-cure*,—an appellation terrific or absurd, as the different “imaginings” of people might make it. But whether terrified or amused by such system, who would not prefer to it the *water-cure* and be ready to exclaim with the poet—though not a very poetical beverage, by the way—*Ἀγιστον μὲν ὕδωρ?*

I may, perhaps, here be excused for going a little out of my path; and if so, I should say that as legislative interference is never desirable, except when called for by the *most absolute necessity of the case*, I would not wish even to see a law framed for obliging medical men to discontinue the use of poisonous minerals in their prescriptions. It is better to reform all this by *national education*; by

teaching the elements of chemistry, of anatomy, and physiology, in our schools ; and by making such knowledge necessary to the taking even of a bachelor of arts' degree in universities. I repeat, it seems better to have no law on the subject ; but if we must come to law-making, I think it would be more for the interest of the population at large to have such a law in operation, than a law which obliged every person to resort only to a certain privileged class of practitioners : or I should say, if legislators will have this latter law, let them by all means have the former as a salutary check on the power they have conferred. Not only individual interests seem to require this, but even the country itself, which is dependent for its existence on the physical strength and good constitution of the male part of the population. And if asked why such are my opinions, I reply on account of the COMPLETE UNCERTAINTY of physic, and the bad effects of corporate bodies commonly—I will not say universally—on the progress of the sciences. Who have most opposed *all examination* into the truth of mesmerism ? Undoubtedly legally qualified medical men. Who at this moment, while I write, have been receiving medals for their scientific discoveries to which they were not entitled ? Fellows of the Royal Society, bound together into a *clique* by ties of interested friendship, as the *exposé* of their proceedings just made by the Literary Gazette and other journals sufficiently shews. The calm consideration of these unjust proceedings, and a firm conviction of the

gross injustice of many writers, and the ignorance of others, have induced me to enumerate my humble labours on the title-page of the present work. When a discovery is of that *practical* importance that it brings its own pecuniary reward, a man may look more calmly on the *clique* that indirectly attempt to give its merit to another: not so in those discoveries which are not only attended with expense but also with great labour, and of which the almost only reward ever must be the inward consciousness to a man that he has added his mite to the amount of human knowledge! To compare small things with great: the people of Athens, in their reply to Pericles, rightly took the credit of what they themselves had done, and directed *their* name to be inscribed on the temples they themselves had reared.

16 Wellington Road, St. John's Wood,
May 1846.

AN EPITOME
OF THE
SYSTEM OF LUIGI CORNARO,
&c. &c.

INTRODUCTION.

MANY eminent men, as Montaigne, Petrarch, Molière, Voltaire, Rousseau, and others, have looked with contempt on the practice of physic, and one of them has not very inaptly said something to the effect that it is the art of putting drugs of which we know nothing, into the body of which we know less. Perhaps it often may not unaptly be called the art of attempting to cure the ill itself has caused. Certain it is the poor vital principle has often double work put on it by even the *cura medici*, setting aside the "*nimia cura medici*." It is compelled to work to cure the disease, and also that unfortunately often still greater caused by the physic given for its cure. Even our Gallic neighbours (though their line of practice generally is mild) are not altogether undeserving of this censure, for they are often careful to prohibit "*poisson*" as food, yet, at the same time, are giving "*poison*" for the cure of disease! to the same person.

We need not look to the stars above for signs of the times, they are sufficiently indicated on the earth below.

Mesmerism, Homœopathy, Hydropathy, &c. &c., clearly enough shew that the public at large are participating, more or less, in the opinion of the illustrious names above quoted. But, alas! for poor human nature, its necessities or its infirmities are destined ever to trample on its spirit and its reason! Those who the most ridicule physic are often those the soonest compelled by their fears or their pains to resort to doctors for succour.

“ When we’re ill we call them to attend us
Without the least propensity to jeer.”

There must and will then always, no doubt, be some form of physic. It is as necessary for the existence (generally speaking) of the body as religion is for the soul, and both of them, in some form or another, ever have and ever will exist, let Reason suggest or decide what she will; and the only question to determine in either case is, what is the best form of each. In physic this will be found no easy matter. Men brought up in the old school must almost necessarily go to their grave in the practice of such principles, and probably we cannot blame them so much for this as we might at first sight conceive; since having seen diseases disappear under a certain line of treatment, they naturally enough have confidence in such treatment; and some often go farther, and have confidence only in such treatment. Now on the principles laid down in this essay they are justified only in the first opinion when they have used no poisons in their practice, and consequently not relieved present ill by injuring the constitution, or by producing remote disease, and consequently disease of unknown and unsuspected origin. In the second opinion I need scarcely say they are never justified.

To correct the error above mentioned what is wanted now is, that students in medicine should be required to

attend a reformed course of medical practice in hydro-pathy, in which no drugs at all are used ; the system laid down in this essay, in which no poisons are used ; or homœopathy, in which they are used in infinitesimal doses. Assuredly, *many* of the rules prescribed by Homœopathy, such as using only one medicine at a time in a prescription, are pregnant with sound sense. It is quite time that prescriptions with six or eight different substances—one to promote perspiration, one to allay the tickling about the larynx in case of cough, one to diminish the heart's action, &c. &c.—were no more, for assuredly new *properties*, totally different from those supposed, generally arise from these heterogeneous combinations. I say, then, that it is but fair that medical students should be required to know something of other systems than the prevailing one, in order to keep their mind free from the prejudices of the old school, which may be justly ranked among the “idols” of Bacon. I have myself, on seeing bleeding to a great extent apparently relieve an affection of the lungs in an old person, conceived that such was the proper plan to adopt in case of a recurrence of the malady. But I was undoubtedly in error, for in a subsequent attack some months afterwards the symptoms all disappeared by a gentle system of purgation and regularity in diet, and the old person had not that *long* convalescence from debility attending the loss of blood, which was inevitable in the first case. Such debility may, indeed, almost be called a disease in itself. Now if I had never seen this last system tried I should certainly have adopted the first a second time ; but in doing so, I should certainly not have adopted a good system, though *apparently* so efficacious. In like manner, with regard to myself, when troubled with a slight diarrhœa, if it does not yield to rice for diet, and increasing the cutaneous action almost to sweating by

exercise, I formerly used to resort to "the chalk mixture" in addition; but I have since found it yield equally well to pulp of tamarinds mixed with water, much used on the Continent, and not leaving that degree of constipation behind caused by chalk.* Diseases may, no doubt, be cured by much milder means than those too generally used, and it is our duty to try these, and not to think the plans of the old school are the correct ones because diseases yield, or appear to yield, to them. Reform in the practice of physic itself is wanted, and without this, reforms in charters will do no good whatever to the public.

* I do not mean by this that chalk should be banished from the practice of physic, for it is admitted by the rules laid down in this essay, as it forms a large component part of the body. My object above was only to illustrate the principle that the same diseases disappear under different, or opposite, systems.

AN EPITOME,

§c. §c.

CORNARO tells us, that though expected to die at forty from the effects of dissipation, he yet, by his system, attained to the age of ninety-eight, free from the usual infirmities, viz. defect of vision, hearing, melancholy, gout, and, in short, all *those diseases to which old men are commonly subject*. He wrote a comedy when above eighty (perhaps not of much merit, for it is lost), and seems to have died only from gradual decay of strength.* It will consequently be worth while briefly to describe what this system was (for certainly the result shews it to have been the correct one), and then to endeavour to see if we can reconcile it with what reason would seem to teach us to be "following nature."

In the first place, he tells us, he found by experiments the common adage—viz. that what is proper for the constitution pleases the palate—to be erroneous, for that he was very fond of very cold wine and rough wine (*approaching* sour, "brusco"), of melons and other fruits, of fish, pork,† pastry, vegetable soups, all which, however, he found injurious to him, or indigestible. But not only in the *quality* of food, but also in the *quantity*, he found it necessary to act in opposition to his own desires, or the dictates of appetite, for his golden rule was,

* Delle Lodi di Luigi Cornaro. Discorso da B. Gamba, p. 163.

† Cornaro della Vita Sobria, p. 18. "E così i melloni," &c.

never completely to satisfy his appetite or thirst at meals (pp. 19 and 25).

In other respects he seems not to have acted in opposition to his own desires, or what may be called the instincts of the vital principle. Thus he says (p. 20) he always endeavoured to let nothing diminish his usual quantity of sleep (without telling what this was). And, be it observed, he did not sleep directly after dinner, for, says he, the small quantity of meat he took gave no tendency to sleep.* He avoided excessive exercise, excessive venery, exposure to winds and *sun* (as he lived in the hot climate of Padua).

There is very little regularity in his writing, though there was so much in his mode of living, and accordingly we have to turn to page 66 (near the end of his *third* discourse) to see what he lived on.† There we learn he took veal and mutton, all sorts of poultry, partridges, thrushes, and among salted fish the dory (*orata*), and “the like,” and of fresh-water fish the pike (*luccio*), and “the like;” bread, light broths with egg (*brodetto con uovo*), “and similar kinds.” We observe in this place that he took some species of fish, though his remark first quoted would have led us to believe he took no fish at all. He took very little, if any, vegetables or fruits, obviously.

* Discourse 3d, p. 66. Writing after dinner, he said, did not hurt him; but, no doubt, he did not take any *violent* exercise after that meal.

† He tells us in another part of the work that he rejected a few things, vegetables, fruits, &c., because he found *by experience* they did not agree with him, and says that every man must be his own doctor in this respect, as there are certain peculiarities in all constitutions, and where no sorts of food seem to disagree, then a person has only to regulate the *quantity*. The author has found radishes to be very indigestible in his own case; for six or eight hours after having eaten them he is subject to flatulent eructations, in which the taste or smell of the radish is very perceptible. There can be no surer sign of the indigestibility of a substance than this. He consequently abstains from them, except in very small quantity.

He also abstained from wine above a year old and pepper, having found them injurious to him. He used cinnamon in place of pepper (p. 29).

By turning again to p. 26 we shall find the *quantity* of food that he took to amount only to 12 oz.; and of wine, for be it also observed he took wine (which he calls the milk of old age) to 14 oz.—no doubt the light “vin ordinaire” of the country. He does not state whether the above quantity of food was his allowance per day, but as the quantity of wine no doubt was, we may infer he ate no more than 12 oz. a-day.

So much does he insist on not taking more than this, that he tells us by increasing his food to 14 oz. and his wine to 16 oz. he was after ten days attacked by violent illness attended with fever, which lasted thirty-five days and nearly killed him. He states that he was seventy-eight at this time, and that he declined to be bled or take any medicines, contrary to the advice of the doctors, and that he depended alone on the strength of his constitution (increased by so many years' regular living) and recovered.

He lays it down as a rule that the older a man gets the less he should eat at a meal—that he should divide the food he has been accustomed to take in two meals into three or four meals; for, says he, the stomach getting older cannot do so much work (pp. 65 and 66).

He insists a great deal on regularity of habits, and consequently, although I do not observe he expressly states it, we may infer that he generally retired to rest at the same hour, and also took his meals at fixed periods. It is, indeed, to be regretted that the work is not written in a more precise and philosophical manner, for he has omitted many particulars that would have been very useful to the physiologist in arriving at certain conclusions.

On reviewing Cornaro's plan of life the only general principle we can arrive at seems to be, that every one should experiment for himself as to the quantity of food he can take and yet leave off with a certain degree of appetite; in like way individual experience is also to be our guide as to the quality, for Cornaro twice over repeats his maxim, "L'uomo non puo esser medico perfetto d'altri fuor che di se solo" (p. 28)—Man can be a *perfect* physician but for himself alone.

There is, indeed, in the beginning of his treatise an assertion already noticed, that the wine and food which pleases the palate the most is not the most suitable for man, but at p. 114 (in his letter to Barbaro) he tells us that from towards the end of July through *the whole* of August he could drink wine of no sort at all, come from what country it might and be made from what grape it might, and that wine at that period was as disagreeable to his palate as it was hurtful to his stomach* ("vino oltre che si fa a tal tempo tutto contrario et nemico del gusto mio, mi nuoce allo stomacho"). Here then, it seems, his apparent general principle, that the palate should not be consulted in such cases, falls to the ground, for wine at such time was both disagreeable to his palate and hurtful to his constitution.

Let us now proceed to inquire whether there are some general principles in this case, and whether we should always oppose or always follow the suggestions of the palate in regard to food and drink; and, secondly, whether we should consult in the same way, or oppose, our desires as regards the duration of sleep.

As we can neither do without food nor without sleep

* He diminished his quantity of food (very properly) in this month as he could take no wine, but he says the debility thereby produced was very great.

—as the *total* absence of either would be certain death— it necessarily follows that nature is acting for the conservation of life in prompting us to these acts. The question, then, we have to answer resolves itself into this,—Should we attempt to *diminish* the quantity of food and sleep that nature seems to prompt us to take?

As regards food, Cornaro would say Yes; but I think when we consider this point more profoundly, we should rather be inclined to reply in the negative. It is true that, if we merely consult the state of the appetite *while eating*, we should agree with Cornaro; but this does not seem the manner in which we should regard this point. Nature has obviously intended that *experience* as well as instinct should guide us as to what is necessary for our conservation. Even the lower animals derive a great deal of knowledge from this source. If, then, we consider the question under consideration in this way, we shall find that by following instinct and experience combined we shall be following nature, for although we may feel a desire to eat more while eating, still, if we wait half an hour or so we shall generally find this desire to go off. Experience, then, aiding instinct (for it is clear that instinct alone is acting on the right side in prompting us to eat too much rather than not enough) seems to teach us not to eat until we feel entirely satisfied.

The same way of viewing the subject seems to apply to sleep, except that it seems doubtful whether experience would ever prompt us to attempt to shorten the period we feel inclined to sleep. No doubt a person could live with less sleep than Nature inclines him to take, but it seems questionable whether he would live as long, if instinct in this case is checked, as if left entirely to itself. Cornaro is quite silent in respect to diminishing the amount of sleep.

It must, therefore, be concluded that the system of

Cornaro is not empirical, but is such as reason or philosophy would teach. But, at the same time that we agree with his conclusions, we cannot agree with his manner of reasoning. We have already alluded to this as regards *quantity* of food, and the same remark applies to *quality*. In the last quotation we find he admitted, in regard to wine, that his palate told him what was best for him ; but not in regard to *food*. But here, again, we must look a little beyond the surface, and mount up nearer to the *state of nature*. That Nature tells animals what food is best for them is obvious. It would have been perfectly useless to have given them an unconquerable desire to eat, if she had not at the same time told them, as clearly as she could do, what food they should eat, and what they should not. We say as clearly as she could do, because she is necessarily governed by *general rules* in this, as in all other such cases. It is undoubtedly necessary for the conservation of life that they should eat, and eat only of such and such things (nutritious substances) ; experience is given to teach them what little more than this they want to know, viz. *which of such nutritive substances agrees best with them* ; and, no doubt, in a state of nature, with ample opportunities of choice, they would fix on those substances which are the best calculated to *protract their life the longest* : for as this is one of Nature's great objects, it would be absurd to suppose she should fail in this, when she has succeeded so well in the other nearly allied to it, viz. the propagation and continuance of the species. Accordingly, the same reasoning will apply to man. Experience (or memory) will aid him in his selection of food ; but he never could have done with this power alone, and, therefore, the instinct of the palate was purposely *given that it should be used*, subject, in a certain degree, to such experience. In the artificial mode of life of society, no doubt, this

instinct of the palate has lost something of its force as our guide (and so far Cornaro is right) ; but it is not altogether inefficient. If it were, why does it not urge us to fill our stomach with earth or sand, or other substance without nutriment ? Cornaro could, no doubt, have come to digest by habit* a small quantity of fruits, vegetables, &c. (just as John Hunter's eagle did to live on bread) ; and, therefore, his palate was not altogether wrong. I, however, by no means wish to diminish the power of experience ; for if we consider that we can only tell what vegetables are poisons by experience and not by the palate, we shall at once see that principle comes to have great influence : more especially is this the case, when we remember that the parents of all animals much assist their young in the selection of food. But as without a desire to eat all this instruction on the part of the parent would be useless, we are still brought back to the

* Cornaro, we have already observed, insisted on the necessity for regularity in *all habits*, as regards quantity and *times* of sleeping, of eating, &c. &c. Any person who, being accustomed to go to bed at ten, sits up to two or three in the morning, will find his faculties to be somewhat weakened the following day. Similar derangement occurs if we suddenly alter our *times* of eating. Where it is out of a person's power to lead a regular life, he ought to endeavour so to arrange as to make all sorts of alterations as gradually as possible. The system may thus be brought to thrive even under irregular habits, and judging from the advanced age that many military men attain, to support such system without any very obvious injury. So completely is life under the influence of habit, that the *cessation* of a noise is known to wake some accustomed to sleep in a noise. In like way on digestion. A labourer in France having met with a serious accident, required, during his convalescence, the most nutritious sort of diet. Accordingly, mutton, chickens, best wine, &c., were given ; but his strength still declining, the surgeon suggested the trial of the diet he had always been used to in his cottage, viz. barley-bread, cabbage-soup, lard, farine, &c. After fifteen days he recovered his strength greatly, and in a month completely (Tissot on Diet, quoted from Ricard, "Physiologie du Magnétiseur," p. 188. Paris, 1844).

instincts of the palate as a guide given by Nature for the selection of food.

We have at present only considered the instincts prompting to sleep, eating, and drinking, in a *healthy* state of the system; and if we have found reason to think that experience, in the case of eating and drinking at least, plays a most important part, every consideration surely would incline us to believe that in a state of disease this same experience should be regarded with still greater reverence.

Is it right, when attacked by vomiting or purging, to attempt to stop these or not? Or is it right even to favour such acts of Nature for apparently throwing off noxious substances or fluids from the system? If the person is nearly in a state of health—in which case the instincts of Nature, as to what is hurtful to the constitution, may be depended on—the best way probably is, rather to encourage than to check the attempt of Nature at expelling such substances from the system. Thus, poisonous and hurtful substances generally tend first to produce vomiting or purging in the healthy subject. But when the vital principle seems by disease to lose some of the fineness of perception essential to its healthy action, then experience will often counsel us to check such actions, as continuing only by the force of habit on the constitution. Thus it is well known that a woman who has had one miscarriage is apt to have another at her next confinement. In truth, habit seems to act on the vital principle, just as it does on the actions (dancing, instrumental music, &c.) under the domain of the will, to which the tendency becomes every time greater by repetition. In such cases as these, Experience comes forward, and tells us at once that the instinct of Nature is no longer to be relied on,—that she, Experience, has been purposely given by Nature to supply the place of instinct.

But *how* does experience authorise us to stop such a vomiting or purging?

By the means the least injurious to the constitution. By promoting *the action of the skin* by the cold-water or any other system. It is only a superficial experience—an experience that regards not *the future longevity of the subject*, that could induce us to resort to the use of POISONOUS drugs to stop the actions in question.

Does not the use of such drugs, especially in large quantities, always leave, more or less, ill effects behind—always tend to shorten life more or less? As no physician can say they do not, he is surely not authorised to use them.

If direct poisons, then, should be excluded from the practice of medicine, what substance would it seem not unreasonable to admit into the Pharmacopœia?

Surely the mineral substances *that Nature herself makes use of* cannot be deleterious. Let us then see what these are in the human body. The principal are the following:—

Carbonate of Lime.	Oxyde of Iron.
Fluate of Lime.	Lactic Acid, or Acetic Acid. }
Phosphate of Lime.	Carbonic Acid.* }
Carbonate of Magnesia.	Sulphur. } Existing in Phosphorus. } eggs and the albumen of blood.
Carbonate of Soda, and probably	
Carbonate of Potass.	Oxygen. }
Muriate of Soda.	Hydrogen. } Constituents Nitrogen. } of animal food.
Sulphate of Soda.	Carbon. }
Sulphate of Lime.	

* This, as a *gas*, is a well-known poison; but, in small quantities in solution, a cooling beverage known as soda-water. Carbonic acid probably exists in many vegetable juices. Besides, be it remembered, it is one thing to breathe it, another to take it in solution into the stomach, as we recommend it. The poison of the viper so taken is harmless.

Most or all of these substances are found in our food, and it is because they are found there, and because they are found as *component parts* of the body, that they are mentioned. We say *component parts* of the body, because there are a variety of other substances found in the *excretions* of the body, which reason would not seem to authorise us to use, except in very small quantities and experimentally. Such are uric acid, urea, &c.

As there would appear to be no harm in using the component parts of the substances enumerated separately, *but in the very small proportions in which they exist in the human body,** the number of substances employed in such system of physic becomes more than doubled. Thus we have the acids carbonic, phosphoric, sulphuric, muriatic, fluoric, lactic, or acetic; and the *radicals* of these, carbon, phosphorus, sulphur (fluorine?), chlorine, oxygen, hydrogen, and nitrogen (as one of the elements of animal food). Sea-water and mineral-waters may, probably, be added with reason to this list of medicaments, since (with the

* This view of the subject makes the practice of medicine, as regards these ingredients, very similar to the homœopathic, though proposed on totally different grounds. There is also another feature in which the present system has some resemblance to the homœopathic, I mean as regards encouraging or tending to increase *first* symptoms of diseases that manifest themselves. Thus it has been already remarked, that when vomiting or purging occur in an otherwise healthy subject, we should give those substances which tend to produce such actions till the stomach or alimentary canal is thoroughly cleared out. *But after this is done*, we would not continue (as we presume the regular homœopathist would) the use of medicines tending to keep up such actions, but rather the contrary, since, as we have already stated, such symptoms may often keep up from the effect of a bad habit (so to speak) the vital principle has got into. The homœopathists are probably right in attempting to increase all *first symptoms*, though they do it on principles different from those advocated in this essay. As a general principle, their system has many advantages over the old Allopathic one; and one obvious one is, that they do not allow themselves to do so much harm as is constantly being done by many practitioners of the old school.

exception of bromine and iodine in small quantities) they seldom contain any ingredients that are not found as a necessary part of the saline elements of the human body. They seem natural purgatives.

On reviewing the number of medicaments allowed for the practice of medicine on this system, we believe that all are included that have really been found indispensable for the cure of disease. We see that sulphur is among them; and this medicine (useful internally for hæmorrhoids) approaches probably the nearest to a specific of any, when used externally, for the itch. Mercury (after having, no doubt, often much aggravated syphilis, and ruined the constitutions of thousands) has at last been found out to have no specific power over that complaint, or at all events not to be essentially necessary to its cure. The unassisted powers of nature, aided by those necessary adjuncts in most diseases, abstinence and rest, have been found fully competent to eradicate this long-supposed, without mercury, incurable malady. And without denying the other famed specific, quinine, a power of stopping intermittent fever, we believe that complaint can be got rid of by other preferable means. More especially does this remark apply to arsenic, which is not equal to quinine in efficacy, and never would have been used in intermittent fever at all, but from total thoughtlessness on the part of medical men as to the permanent injury such a powerful poison would leave behind in the system. The use of this medicine in intermittent fevers was as unjustifiable as the use of nitrate of silver (lunar caustic) for the cure of epilepsy. Although there is a gentleman to be seen whose skin has been permanently and irretrievably discoloured by the use of this poison, so that looking more like a corpse than a living being, he is rendered unfit for general society, still, we believe that some medical men

are still to be found who would not hesitate to give it, as they say, in small doses! as if, when even it leaves no such *external* disfiguration behind, it does not leave an equally permanent *internal*, invisible, irremediable one, of its own creation!

Having attempted to lay down a general rule with regard to the use of mineral substances in medicine, it becomes us to state what restriction we would put on the use of *vegetable substances*. As a general rule these are preferable to mineral substances not component parts of the body. Thus, an emetic of ipecacuanha is preferable to tartar emetic, sulphate of zinc, or sulphate of copper, which three last ought certainly never to have been introduced into medicine at all; for what can have been a greater absurdity when vomiting can be produced by tickling the fauces, drinking hot water, or at all events, by a solution of common salt or ipecacuanha, to seek for other means of effecting the same end, and that too by, I had almost said, such murderous means *as poisons*!

Although there is little reason to believe that vegetable medicines can produce such injurious effects on the constitution as mineral substances (not existing as a part of the system), still we shall be on the safest side to use as few of these as possible, which are not found *capable of serving as food*. A purgative substance exists in spinach, a stomachic one in the hop (lupulin), and an anodyne one in lettuce (extract of lactucarium); and so of some other vegetable substances. Vegetable medicines of this class then seem the surest; and there is little doubt if this extensive field is investigated we shall have many more useful discoveries in medicine, than by trying the virtues of such powerful poisons as strychnine, prussic acid, &c. The rule which I should be inclined to lay down in this case would be, to exclude vegetable *poisons* (except, perhaps, in homœopathic doses) from the

practice of physic, but not other vegetable substances, as senna, &c. &c., but only to prefer those which are derived from articles of food.

On the principles just alluded to, animal substances and fluids would appear to merit farther attention than has yet been given them.

What would be the effect, for instance, of small quantities of rennet (pepsin, or the gastric juice, preserved by salt) in cases of indigestion, that are not alleviated by the usual routine of vegetable bitter infusions? Or would it be wrong to use the rennet or gastric juice of any but of the *same species* of animal (as we know this is necessary in cases of transfusion of blood)? Although there is little reason to think the rennet to be beneficial should come from our own species, still it may be as well to attend to this point. As digestion is so essential to life, many cases of debility and marasmus, not being accompanied with precisely any obvious symptom of indigestion, may be benefited by this substance, which, in combination with the means now about to be proposed, seems the most rational for attempting to extend life to its utmost extent.

The other means now alluded to are the transfusion of the blood of one of its *full-grown* and healthy offspring, back again to the old parent, at certain intervals, where the parent, as in Cornaro's case, seems on the verge of death from mere loss of vital power. Experiments that have already been made on this subject shew that the blood must be of the *same species* of animal; and hence I have imagined that a still nearer approximation to absolute identity would be advisable. Hence, where there is a family, the blood of one of the healthy adults among these should be preferred to the blood of any person not "connected by blood," as it is called. I need not say how desirable it would be that physiologists should try

this experiment on animals first, and see whether there is not a re-vitalising power in the blood of the adult offspring for its aged parent.* If there be any thing approaching the character of an *elixir vitæ* in nature, it seems more reasonable to look for it in the blood of the adult and healthy offspring than perhaps to any other source. I do not, however, think that the inspiration of oxygen at *the period of old age*, has yet had so satisfactory a trial as to enable us to pronounce on its merits. The effect of this gas, therefore, should also be tried on *aged* animals; for, though fatal to young animals, it does not seem necessarily to follow it should be so to the old; at all events, *if diluted with common air in various propor-*

* "On verra," says Mesmer, truly, "que la marche naturelle et inévitable est de passer de l'état de fluidité à celui de solidité." ("Deuxième Mémoire sur le Magnétisme," p. 78. Paris, 1844. Edit. Ricard.) We have found Cornaro thinking that the old should diminish their quantity of food. Should they not also take it in a less concentrated and solid form, *like infants*, to the nature of which, in fact, they tend to approach? Should they not also combine acid fruits and a proportion of vinegar with it, to tend to prevent this obvious tendency to increased cohesion—to *ossification* of old age? Experience must decide. This tendency to ossification may, perhaps, be considered the physical cause of death in most old persons, who die without apparent disease.

There is in old age, also, a want of reproductive power in the *individual* parts of the body, as well as of the body taken as a whole—as well, in fact, as in the act of generation. Perhaps, indeed, the loss of this latter power is dependent on this loss of generative or reproductive power in the individual parts. However this be, it is certain that there is a tendency of most parts to shrink or be absorbed, and not be again reproduced, as we advance in age. Thus come on wrinkles in the countenance, shrinking in the size of the limbs, gradual and *almost insensible* decay of the teeth, till at last their decay becomes obvious from loss of colour and pain. We may watch this very slow effect on the front teeth especially, in which a gradual loss of substance goes on for years before they exhibit the obvious appearance of decay from *change of colour*. Of course no reproduction here takes place. Is the same not the case with the nerves? Is not the amaurosis and loss of hearing of old persons dependent often on this *loss of substance* (and, perhaps, consequent loss of flexibility) in the optic and acoustic nerves?

tions : also oxygen water, and solution deutoxyd hydrogen. Eggs and oysters, as evidently *containing a vital principle*, may be used as the nutriment in such cases ; for from their exciting nature they seem, perhaps, to impart a certain quantity of this to those who use them as food.

Whether on similar principles there can be a communication of nervous influence, can also only be learned by experiment. Does the optic or frontal nerve of an animal *just killed*, laid on the upper eyelid, benefit amaurosis ? Or the spinal marrow of the same, laid or rubbed on the spine of the debilitated old man, give any additional strength ? I put down these suggestions without being very sanguine as to their results, but yet as appearing worthy of trial.

On similar principles the sleeping of the young with the aged is said to prolong the life of the latter ; which, supposing it a fact, is attempted to be accounted for by the mesmerisers on principles of natural magnetism, but vaguely. However, as the phenomena of mesmerism, undoubtedly true, seem to support the *possibility* of something like a *communication of life by contact*, aided by manipulations ; and since the effect of the vessels of a living animal, in preserving the fluidity of the blood, seems likewise, perhaps, to favour the same doctrine, researches on this question seem worthy of attention.

Although I do not believe that human life would be prolonged to any extent by any of the above means, still it is from such experiments that we are to look for the “aurum potable”—the *elixir vitæ*—as far as it exists, and not to the administration of drugs and minerals.

One point that Cornaro has well considered, as far as possible, is regularity in habits. It should be a general rule not to attempt to alter *habits* at all ; but as this cannot be avoided sometimes, it should be a general rule

never to attempt such alteration, but in the *most gradual manner*. The immense effect of habit on the constitution is shewn by persons gradually enabling themselves to take great quantities of some poisons, as opium for instance, with impunity. If they had attempted any change of this sort *suddenly*, they would inevitably have destroyed themselves.

There seem to be no such authentic records of persons having become enabled to take great quantities of *mineral* poisons with impunity, as of vegetable poisons; for the case of Mithridates is probably not sufficiently well attested for modern conclusions to be founded on. Certainly, in the present state of our knowledge, no person has yet come to bear with apparent impunity (I say *apparent*, for no doubt in all these cases the vital principle suffers more or less) the same quantity of arsenic as of opium. This is all in favour of vegetable poisons (as a general principle) being less injurious than mineral ones. Consisting for the most part, as they do, of the elements of the human body, we should naturally expect they might, in small quantities, gradually come to be decomposed, and be wholly assimilated to the animal textures; in which case they would not, like mineral poisons, leave a fixed metallic basis behind, which, as not existing originally in body, would still require to be ejected by the vital processes.

And here we may pause a moment to consider how truly wonderful it is that vegetable poisons, consisting as they do *solely of the elements of the animal body*, should yet be poisons. Look, for instance, at prussic acid, in which hitherto nothing has been discovered which is not a component part of the simplest animal textures, and yet here we have one of the most quickly fatal poisons of any! Supposing our present chemistry right on this point, one conclusion certainly follows, viz. *that a dif-*

*ference in the arrangement of the atoms of matter is perfectly equivalent to a difference in its original nature, a conclusion arrived at in my experimental inquiry into Liebig's Views on Fermentation (Lancet, 1842). There it is shewn that the principle applied as to common chemistry—as to the solubility of a substance in a fluid menstruum. It was then naturally to be expected that it would apply also to vital actions, and that the vital principle would be affected by peculiarities of the feeblest kind, on account of its presumed great sensibility. Nothing can shew that this is the case more clearly than that the *elements* of food arranged differently become poisons.*

It is usual to call oxygen, hydrogen, carbon, and nitrogen, the food of animals and plants; but the above reflections would justify in saying that these elements, *united only in a particular manner*, can be regarded in the light of food. In fact we might have arrived at this conclusion from considering that excrementitious matters of animals seem still principally to consist of the above elements; yet, of course, are not susceptible of assimilation. Another reflection that at once leads to the importance of the *peculiar manner* in which the elements of food should be combined is, that nothing as yet has been found to serve as food to any extent, except animal or vegetable textures, or that which has *been formed in a peculiar manner*, as yet unknown to common chemistry. This consideration, and the fact that animals all throw off excrementitious matter, and vegetables to some slight extent, since they give off carbonic acid gas,* would almost lead us to conceive that oxygen, hydrogen, carbon, and azote, contain in their uncombined state, *or combined*

* The opinion that they throw off excrementitious matter by the roots, seems to have been rejected shortly after it was proposed.

in any way in which they cannot be used as food, some unknown noxious principles, which must be separated before they can be converted into the animal textures — before they can serve as food.

It does not seem impossible in a more advanced state of chemistry that important discoveries should be made on this point.

These reflections bring us to consider that at some future period the artificial formation of food may not be a mere chimera. We have heard, even at the present day, of fine sawdust, after having been made to undergo fermentation, being formed into a species of bread. It does not seem, therefore, impossible that other organic combinations (now rejected as useless) might also, by means of fermentation, be so *purified* (if the expression may be allowed) as to become palatable, at least for the poorest classes. Nay, it seems not impossible that some decidedly *inorganic* matters, as silica, might, under certain treatment, become nutritious. Silica, in the form of jelly (as precipitated from silicate of potass by an acid), might be tried, mixed in proportions, gradually increasing in young animals, with milk, or in adult animals with their usual food. If it served as nourishment, Dr. Brown's opinion as to its convertibility into carbon would certainly gain strength. If the gelatinous silicate seemed to injure the animals, possibly the use of a ferment mixed with it might tend to alter the arrangement of particles, and render it nutritive. Similar experiments might be tried with carbonate of lime (a large constituent of animal bodies), though neither in this case nor in that of silica does it seem probable that a ferment would be able to change the nature of the substance. Carbonate of lime should be tried with the phosphate, ammoniaco-magnesian phosphate, or some other substance; as alone its astringent properties might altogether injure the

health, by stopping the action of the bowels. However, there seems little reason to believe that chalk could really be made nutritive, except some great changes could be effected in its nature, similar to fermentation. I say similar to fermentation, because a curious fact seems to have been discovered by Boussingault, viz. that if a little butter be mixed with rice, and given to animals as food, it will create a great deal of fat, but rice alone will produce no fat.* Now here some mysterious principle seems to be in operation, — something analogous to fermentation, by which a small portion of a substance *placed in particular circumstances* becomes capable of producing its like, for butter and fat may probably be considered as identical, or nearly so. It will be curious, should it ultimately appear that there is a species of what may be called *organic fermentation*, that is capable of pervading the very living and organised tissues themselves. But the application of this fact to the production of anything analogous in gelatinous silica or chalk seems very doubtful, seeing that rice is an organised substance, or at least a product *actually formed* by organic secretion, and not merely deposited in consequence of *having been taken from without*, as, according to the present state of chemistry, is the case when silica or carbonate of lime form part of the body. However, Boussingault has still more lately found that the ammoniaco-magnesian phosphate hastens the growth of vegetables, so that the effect of gelatinous silica may be tried also on these. If silica ever can be decomposed (I mean still further than into silicon and oxygen), it seems probable it will be by presenting it in the gelatinous form to the animal or vegetable vital processes; since we have already seen, by the example of prussic acid, that *mode of arrangement* has

* Compt. rendus Acad. Sciences, 1845.

quite as much to do with rendering a substance nutritive as composition itself, and silica in the gelatinous state is differently arranged (if a simple substance can be so) from what it is as mere silica powder.

We have already observed that the only *mineral* substances we would employ in medicine are those which form part of the body in its healthy condition ; and we have also added in regard to proportions, and only in the proportion in which they exist in the body. This rule, however, is more worthy, generally speaking, of rigid observance, when we consider the *elements* of the component parts of the body than substances formed from these elements ; of sulphuric, and phosphoric, and fluoric acids, for instance, than of sulphate, or phosphate, or fluuate of lime or soda ; and still more so of sulphur, and phosphorus, and fluorine—the *elements*. I say, generally speaking, because *I do not wish to exclude experience as our best guide in such cases* ; and we know that sulphur seems to pass off more innocuous from the system than sulphuric acid in the same doses. This, however, is not probably the case with phosphorus and fluorine, which are probably more violent poisons than their acids. In this particular case the general rule probably is, that all these simples, as well as their acids, require some neutralisation, as a qualification to enable us to give them (except in homœopathic doses) with impunity. Thus sulphate, phosphate, or, perhaps, even fluuate of soda, are harmless purgatives, while the acids and radicals are poisons ; except the radical sulphur.

It will appear from this that we do not exclude those poisons from our system which form a component part of the human body in its healthy state ; on the contrary, we recommend the trial in infinitesimal doses (at first) of phosphorus, fluorine, fluoric acid, deutoxyd hydrogen (taken immediately before decomposition commences),

cyanogen (which seems to exist as such in the human body), solid or in solution. Carbon itself is already used by the homœopathists; and ammonia (hydrogen and nitrogen), more particularly useful as smelling-salts, or for external application, is also taken internally.

I know that, on looking over the above list, it may be said we do not exclude some of the most powerful poisons from our system; and that in recommending the trial of the effects of cyanogen, we are approaching to prussic acid. I grant it; and admit that while prussic acid and the vegetable poisons should only be used in desperate cases and in homœopathic doses, I certainly would banish arsenic, copper, silver, &c. from the practice of physic even in such doses, because they are elements which do not exist in the human body naturally, whereas all the elements of prussic acid do exist there.

AN EPITOME
OF
M. RASPAIL'S
NEW SYSTEM OF MEDICINE,
(EVERY MAN HIS OWN PHYSICIAN,)
INTERSPERSED WITH COMMENTARIES.

“Ridentem dicere verum,
Quid vetat?”—HOR.

We have heard of embalming after man's death,
When for ever has left him the vital breath.
Thus the dead man is kept, no doubt, from decay,
And all sorts of vermin chased from him away ;
But here in this book of Raspail you will read,
That *during our life* of embalming we've need ;
That insects assail us within and without ;
That from these come all ills—both Fevers and Gout ;
That CAMPHOR and ALOES—these only can save us,
And keep that life sound which great Nature gave us.

* * * * *

We know some love smoking—smoke half the day ;
To all our great smokers here then we say,—
Adopt this new plan, smoke too *all the night*
Your camphor cigar, which ne'er wanteth a light !
Thus by you, *in your life*, no worms will be fed—
Quite enough to be eaten by them *when you're dead !*

Author's MSS.

AN EPITOME
OF
M. RASPAIL'S NEW SYSTEM,
&c. &c.

INTRODUCTION.

THE distinguished philosopher F. V. Raspail has written two works on the present subject—a large one in two volumes, and a small pamphlet, which latter, in particular, has had a most extensive circulation, having passed through sixteen editions. The first work is not only filled with details in natural history, but also contains *proofs* that a vast number of diseases, both internal and external, have arisen from the ova of insects or from the presence of acari.

In reference to these large volumes, the author of the present work begs to say he has only quoted the parts that relate expressly to medicine, and which contain *proofs* of the correctness of M. Raspail's theoretical views. He has considered this will be a useful addition to his small work above alluded to, in which, for the most part, *no proofs* are given that diseases have arisen from “animate contagions,” as Dr. Neale some time ago called them. The author has considered, that if people are to be their own physicians they will require, at least, some

evidence that Raspail's opinion is based on facts. Such facts, therefore, we have selected from his large work and given *in addition* to the leading points of his small work, which is little more than a list of the medicines M. Raspail uses, with the modes of preparing them. It is hoped the English philosophical as well as general reader will, therefore, find all in the present work *necessary to convert him to Raspail's opinion*, and also all that is necessary to enable him to prepare the medicines himself. The original large work of Raspail will be read with interest, more particularly by naturalists, being filled as it is with classifications and entomological details, which the great mass of medical men have not time, and often not inclination to follow. It will also be read with interest by the general scientific reader, since every one is interested in the preservation of his health; and there are in various parts touches of exquisite irony, not unworthy the country of Pascal and of Molière.

In a work in every way original, errors of opinion were to be expected. The author leaves for naturalists to decide, as we shall see hereafter Raspail attempts to prove, whether Mr. Owen has in his discovery of the trichina spiralis discovered (unknown to himself, so to speak) the eggs of the lombricus, and also, whether hydatids are really the eggs of the tape-worm, as Raspail considers. There seems in these cases some difficulty in conceiving how such large worms could have got into the muscles and interior of all parts of the body without leaving any traces behind; and the theory of the respiration of ova in these cases is beset with many difficulties. Yet it is upon these views, as *causes of disease*, that a great part of Raspail's system is founded. This question of abstruse physiology, therefore, now becomes a question of great practical importance. Hydatids, in particular, have hitherto been generally considered only

effects of disease, and physicians have consequently, in such cases, rather aimed at bringing the vital action round to its healthy condition by the use of medicines calculated to correct the state of the digestive and assimilative functions, than at destroying the vitality of these now called ova. The author must, therefore, in the present state of this inquiry, consider that Raspail has too hastily almost expunged digestive medicines (carbonate of soda and bitter infusions) from his system—the secret of Abernethy's success; and also in other cases (which will be found commented on as we proceed) that he carries his doctrine of worms and their ova being the cause of disease too far. Caries of the bones, gout, diabetes, are, according to him, diseases arising from acari, or worms; and if we ask why, we shall be answered *by implication*, because he has given well-authenticated cases where acari *seem* to have been the cause of dysentery, &c. (for we must admit he has given cases of many diseases existing *contemporaneously* with acari and worms). In like way, because he has discovered the acarus which causes the itch, he puts down almost all cutaneous diseases to an animate cause.

In consequence of having perceived this, we have in the following essay commented with freedom on the opinions and facts brought forward by the distinguished republican author (the man, too, who has suffered martyrdom for his political views), and our good opinion of his work is not much diminished by our being often obliged to differ from him; for if he has carried his doctrines a little too far, physicians hitherto have been far more culpable in almost entirely neglecting these *animate* causes of disease. Raspail well deserved the Montyon prize of 10,000 frs., which was adjudged to him for the present work; and though we do not consider he has proved this animate cause of disease in many cases

he has put down, *yet he has rendered it probable by analogical reasoning, particularly in the case of all cutaneous diseases ; sufficiently so, at all events, for us to follow his plan of cure as a general principle.* The almost only alteration in any case that we should be inclined to make in it would be to attend a little more to the digestive organs, giving less aloes for the rectum's sake, and more carbonate of soda and bitter infusions "for the stomach's sake ;" for whenever he uses bitters it seems to be always for the *worms' sake*, be they proved to exist there or not.

Even supposing hydatids, &c. are only *effects* (of some mysterious modification of the vital power by disease), still our object certainly should be to try and destroy their vitality in some degree, though not in so marked a degree as if they were demonstrated causes. We have, therefore, throughout this essay, objected to Raspail's system, in the present state of our knowledge, rather as an error of quantity than of quality. The reader will thus be in a position to judge, after perusing the present publication, whether he thinks it advisable that Raspail's plan should be followed out to the full extent he recommends, or only adopted with the restrictions herein suggested.

Cases of cure speak best for themselves. We have, therefore, carefully selected from his works the most striking cases of success ; and among these will be found cases where his "*eau sédative*" has supplied the place sometimes of leeches, sometimes of bleeding, and sometimes of blisters, the use of all which he condemns. Brought up as we were in the antiphlogistic school, we confess we have read these cases with much interest, tending as they do to shew that the stimulating line of practice is often more rapidly successful than the plan precisely opposite, and in which alone we have hitherto

placed confidence. Such is the effect of education and experience on the mind! We have, however, learned by Raspail's book, that even experience has its errors; that there is a short and strengthening way, and a long and debilitating way, of getting rid of diseases; and that we have hitherto believed but in the latter alone! We must now, however, confess our opinions are changed. The candour with which his cases are given entitles them to our full confidence. We have, indeed, left the doubtful and unsatisfactory cases out of our selection; for as medicines cannot renew old or worn-out organs, such cases must occur whatever line of practice we adopt. But the simplicity with which these are given in the original work, shews that Raspail's only object has been the discovery and propagation of truth. Not taking any fees for his attendance (having no diploma), he has not the motives which many have for giving an *ex-parte* statement. Had he wished to have done so, he would have left out many cases he has inserted.

But adopting Raspail's views as to the *animate* origin of the greater part, if not nine-tenths, of our diseases, as he says, there is another point which we should alter in regard to his preventive and curative measures. We allude to a more general adoption—when aperients or remedies for indigestion are necessary—of *saline* medicines. It will be seen above that carbonate of soda is recommended, along with bitters, when the stomach is out of order. Now the carbonate of soda in this case may not only act as a preventive of flatulence, but also, by entering the circulation, as a corrective of the state of the blood, when such salt exists in it in diminished proportion, or when from other causes its colour is a little darkened. On the same principle we should prefer the use of Seidlitz powders, sulphate or phosphate of soda, or an enema of common salt, when purgatives are required;

since by such means we offer a chance for these salts to be absorbed and mixed with the blood, which we have reason to believe in many cases is defective in the quantity of its saline matter, as it has been proved to be—as will be seen in the body of this work—in the case of cholera. At the same place it will also be rendered probable, that such deficiency in the quantity of saline matter may also be a principal cause of a great many other diseases,—*in fact, all those that have an animate origin.* We cannot, therefore, too strongly recommend that experiments should be made to ascertain whether in cases of worms the quantity of saline matter in the blood is not diminished. And in the present state of our knowledge, as such point seems highly probable, we would rather, for our own part, take a little less aloes and camphor, and a little more saline medicine as *preventives* of worms, than what Raspail recommends. We say as *preventives*, because there is reason to believe that *animated ova, of whatever description, cannot come to maturity in a body sufficiently impregnated with saline matters.* We would not, indeed, say they could either, when our fluids were thoroughly impregnated with camphor (for, like salines, camphor preserves from putrefaction); but then it is to be remembered, that salines—muriates, sulphates, phosphates, and carbonates*—are NATURAL constituents of the blood and living body generally, which is not the case with camphor; and, for what we know, *heterogeneous* substances—substances not naturally found in the body, may always exert more or less deleterious influence, when taken so *habitually* as Raspail recommends. Aloes are used rather for the expulsion of worms come to maturity, than pre-

* The quantity of iron in the blood seems less liable to variation than that of saline matter; but when found defective, a few grains of sulphate of iron may be put in the water we drink at table.

venting their developement ; and, therefore, there is no objection to an occasional use of this, of powder of fern-root, &c. (once every three or four months), in case of illness, to see if the disease have its origin in worms.

It is singular that Raspail did not suggest this system himself, since we shall see, by the following passage, he was as near it as Professor Robinson (to compare small things with great) was to that immortal discovery which was reserved for Volta !

Under the head Goitre (vol. ii. p. 502), he says this disease seems to arise from some parasitic animate cause that forms the tumour (he has not, however, yet discovered any such presumed acarus, or worm). But proceeding to the part immediately to our purpose, viz. the treatment, he says, "The end of the physician in this disease is, to impregnate the tissues with the salts which are in deficiency, and in consequence of which deficiency the parasitic author of this work is enabled to live there." In consequence, in this disease he recommends a mixture of common salt and sulphate of iron to be taken internally, and also alternated with eau sédative applied around the neck.* Whether there is a deficiency in iron and salt in the blood in this disease is a subject worth inquiry, and if there be, his plan of treatment is highly rational. Why, therefore, we may ask, has he not recommended the same in cases where *parasitic life has been PROVED to exist*, as in cases of intestinal worms, hydatids, &c. ? Or in all those nine-tenths of diseases which he conceives to arise from such causes, or,

* On looking at the article Goitre in the "Méd. Soi-meme" (p. 192), we see that the internal use of salt and iron is not mentioned. On what grounds, therefore, he has insinuated in his large work that these are defective in the blood of such patients, we do not know. Be he right or wrong in this case, the bearing of the observations in the text is not affected.

at least, that large part which arises from acari, or the *undeveloped ova* of ascarides ?

We can only ascribe this to an oversight, and believe that he would, were the subject broached to him, recommend a careful analysis of the saline matters of the blood in all diseases ; and the administration of iron and saline matters in all cases where a deficiency of these was proved to exist.

The fatal result of many cases where the injection of saline matters into the blood in cholera was tried, shews that we ought never by such violent measures to attempt to cure diseases arising from a deficiency of these matters in the system ; for, setting aside the danger from inflammation of the veins (an important consideration), to attempt to increase salts in the body by such injections is beginning at the wrong end, and thus is sure to excite and harass (so to speak) the vital principle, which is always accustomed to select the proper quantity of salt necessary for health from our food, and to force it to pass by the way of the lacteals or absorbents only into the blood. Hence salines should be administered by way of injection, or by the stomach ; and in *extreme* cases, given in strong camphor-water, or along with the powerful vegetable condiments or anthelmintics recommended by Raspail.

The propriety of attempting in this way to *keep the composition of the body always the same as it is in health*, cannot be too strongly insisted on. *In every disease there must be more or less change in the chemical composition of the body*,—sometimes in regard to its saline constituents, sometimes in regard to its elementary atoms ; and the great object of the physician should be to discover and remedy this cause of derangement, since the latest chemical researches prove that the chemical powers of the vital principle are far more limited than might

have been inferred from Dr. Prout's paper in the "Philosophical Transactions" for 1822, and other previous works of various authors. *If the proper materials are not presented to the vital principle* by way of food (and in some diseases it may require more azote, in others more sulphur, in others more phosphorus, &c.), as it cannot form these so-called elements, the body must necessarily become diseased. It is, therefore, to be hoped that organic chemistry will henceforth be made essential to medical practice, which, to be based on scientific principles, requires not only the blood, urine, &c., to be analysed, but also the food itself.

It seems highly probable that if there were no fault in the composition of the body, hydatids or worms could never prey upon us—at all events so as to injure our health or longevity; for otherwise we should have been made in vain, which is inconsistent with what we see. All animals (and man among them) are so formed, that by following the system of diet which Nature points out to them, they are able to attain to old age, at which they die only from a loss of power in the vital principle. Nature herself indicates the use of salt, of condiments, and, perhaps, to a certain extent, alcoholic liquors. Now these may be called Raspail's *natural anthelmintics*. We must therefore repeat, that unless disease has actually appeared, the *habitual* use of camphor and aloes seems to be a work of supererogation not to be recommended, *since they are not constituents of the body in a state of health*. Let us not give any reason for inscribing on our tombstones, "I was well, I wanted to be better for fear of the worms, and now here I am!"—come, in fact, to them before his time. Camphor and aloes, are, no doubt, *very* slow poisons; but it is as well not to run even the chances of having five or ten years cut off from our lives when there is no necessity for it. That the reader

may perceive this opinion of the camphor system carried to the *full extent* recommended by Raspail, even to persons in health, is not too severe, it is to be remembered that Raspail smokes camphor all day (sometimes, perhaps, all night), that he always sleeps with a quantity of it under his bed, and that he has taken *fifteen grains* (!) a-day for the last five years, and a dose of aloes almost every week; and all this to *prevent* disease. Could he, we might ask, do much more if it were there, and he wanted to cure it?

His opinion of our taking in the eggs of ascarides by respiration deserves consideration. Spallanzani and other naturalists have conceived that the eggs of *animalcules*, or animalcules themselves, float about in a dry state in the air, and are developed when they fall into a suitable *nidus*. On this theory the air may be said to be full of life, and it may be asked, May not the eggs of the infusoria be useful in supporting *our* vitality? May they not be taken in by respiration, and fixed in the tissues as organic globules, and thus made part of the living body, and never in such case pass into the state of animalcules with independent voluntary motion? Though the eggs of ascarides be deleterious to our life, the eggs of animalcules, or *dried* animalcules themselves, which come to life on being moistened, may not be so. To throw light on these questions experiments are wanted, on the effect of air which has been left some time in contact with strong sulphuric acid, or caustic potass solution, on the life of the lower animals; these might be confined in portions of such air for some days together. If the animals did not live, it might reasonably be presumed that *the vitality of the organic matters in the air* was necessary to our life. Hence the smoking of camphor with the idea of destroying such vitality would not appear in such event a plan to be recommended. But the experiment is yet to be made.

SECTION I.

ORIGIN OF DISEASES.

RASPAIL admits but a *material* origin of all diseases, or of by far the greater portion. Such material causes he divides into two classes: 1. Inorganic; and, 2. Organic, or Animate; in each case the causes in question being, *generally* speaking, too minute to be discerned by the naked eye.

Inorganic Causes.—Let us consider these first. Some irritant, says he, is taken in *with our food*,* which causes gastritis (inflammation of the stomach), or enteritis (inflammation of the intestines); if taken in *by respiration*, either catarrh, asthma, croup, inflammation of the lungs, or pleura, or consumption (phthisis). If the same irritant be carried to the liver, heart, kidneys, we shall have diseases of these organs. He thinks palsy and sciatica arise from similar irritants conveyed and lodged in particular nerves; that diseases of the uterus and vagina often arise from such mechanical irritants carried there by the air.†

* “*Histoire Naturelle de la Santé et de la Maladie*,” tom. i. p. 230, Paris, 1845. I purposely have begun my commentary at this part of the work, as being the commencement of the *practical* portion of it.

† Raspail rather directs the attention of physicians to such inorganic causes, than details cases. No doubt fine metallic particles, &c. floating in the air have caused many diseases, as the pulmonic diseases of people employed in large cutleries, &c., shew.

Organic Causes.—These he divides into two classes: 1. Those that cause diseases only by germination or developement; 2. Those which disorganise the tissues by reproducing themselves. In the subsequent chapters he brings forward evidence to shew that the first division of these organic causes has occasionally produced diseases; and hence he fairly enough infers they may have caused many more, if a careful attention to the study of causes had been given. At p. 235, he quotes a case to shew that a grain of barley has germinated in the stomach, and caused fever and frequent retchings, the cause of which remained mistaken, till the barley, in a state of germination, was dislodged from the coats of the stomach, into which it is presumed to have penetrated. In like manner, a tumour of the nose of a child was supposed to arise from a polypus, till a pea which had germinated in the nose was extracted. Another case is given where intense pains of the right side of the head arose from a bean, which had remained in the ear many years unknown to the patient; and at p. 366 another similar case is given, which was cured when numbers of worms, like the mites of cheese, were driven out by injecting goat's milk.

But it is to the second division of organic causes, "which act not only by their developement, but also by the mechanical and destructive action of their nutrition" (p. 337), that we are especially to look for the full developement of Raspail's theory, illustrated by numerous drawings of such acari, and—what is of more importance for his argument—exemplified by cases where constitutional diseases have actually existed at the precise period such acari were detected, either boring into the skin and causing cutaneous diseases, or *ejected amidst the excrements at the time the patient was very ill with dysentery* (p. 367). We will take this case first, as it so

stands in the author's work, before proceeding to his remarks on the acari, &c., producing other diseases.

The patient, in this case, being a naturalist, discovered by severe scrutiny in his fæces thousands of the acari of flour. On further inquiry as to their origin, he discovered that the wood pitcher (*cruche*) with water in which the patient used to moisten his mouth during the night had a crack in it, and in this crack he detected myriads of these acari, which, says Raspail, no doubt left this crack during the night to get into the sick man's mouth and elsewhere, and returned again at day to their hiding-place. It was found by experiment that these acari perished immediately in spirits of wine, or the juice of rhubarb, and it was by rhubarb chiefly that the cure of the dysentery was effected.

The acari in question, says Raspail, like those of decayed cheese, are capable of living in *heated* flour; and, as we have seen, can live in the intestines in consequence of their tenacity of life. They live in the joinings of *old* furniture, and in houses old or dirty, old wax, collections of dried plants or animals, in ill-conditioned ulcers, and wherever the *fermentation caséique* can develop itself. Hence it would appear right that the joinings of *all old bedsteads* should be well examined not only on account of bugs, but also of these acari; and, no doubt, it is chiefly to prevent the entrance of these into the mouth or nostrils, that Raspail uses his cigarette of camphor *during the night*, when, as we have observed, they generally begin to move about.*

* This acarus is white, and covered over with hairs or bristles (see Raspail's second plate, Nos. 13 and 14). It is certainly much like Mr. Cross's acarus. Raspail, however, says that the acarus in question lays *eggs* even under the microscope (p. 365). Now the mode of propagation of Mr. Cross's acarus is certainly *by division*, just as Spallanzani tells us is the case with most animalcules. In specimens kindly forwarded me

It has been long known that the *œstrus ovis* (Lin.) lays her eggs in the nostrils of sheep. Raspail quotes from Razoux a case of violent pains in the head, arising from the presence of seventy-two of these same worms, in the nasal fossæ of a woman, which are so common in sheep. The pains left her as soon as these worms were discharged. The woman had gained these worms by drinking largely at a muddy pond ("en s'abreuvant à une mare d'eau bourbeuse"), where sheep were accustomed to drink (vol. ii. p. 50).

Farther on we have a case of large tumour of the hip, from which, when open, 4000 worms (some large, others small and long) were discharged *alive*,—probably, says Raspail, the larvæ of the same species as the last.

By sleeping, says Raspail, near where sheep or horses are in the fields in the day-time, we expose ourselves to the visits of this same *œstrus*, which can live in any part of the intestinal canal or nasal fossæ. If a person so circumstanced were to die from the effects of the larvæ, dissection might never discover the cause of the disease, for these larvæ only seek living bodies, and hence might have been discharged with the fæces previous to death, or been decomposed and killed themselves by the putrid fermentation or fever they had caused in this same human subject.

Another case is given where a polypus of the *uterus* which required extirpation seems to have arisen from an *œstrus* having deposited her ova there (p. 50). Clot

by that well-known philosopher and surgeon, Mr. Weekes, which I kept many days, I observed one slowly day after day to become larger, and at last a division appeared in its centre, which gradually contracted, till at last two insects arose from one. I invite naturalists to a more careful study of the mode of propagation of the *acarus* of cheese and flour, since some persons seem to have considered this identical with Mr. Cross's *acarus*.

Bey, in his work on Egypt, says that Egyptian women are far less liable to *écoulements* and uterine diseases generally than European women, which he ascribes to the more frequent use in Egypt among the women of muslin drawers, whereby so free an access of the external air is prevented. He only looked to the difference of temperature of the air from the human body as the cause of disease: on Raspail's views we may recommend Clot Bey's *preventive* also on other grounds. At pp. 58 and 59, we have cases where the larvæ of flies were voided with the urine; from the nose of a woman, in 1818, to the number of 1300, who had slept out in the fields, and whom, probably, the fly had mistaken for a corpse; from a tumour, when opened, in the limb of a lady, which had swelled to a monstrous size; from all parts of the skin of a young man, the substance of the left eye having been quite devoured by them, and also large places in the ham (*jarret*) and anus. In this last case the patient died and not a worm was found in the intestines. In 1826 a similar case occurred in Paris, in consequence of the patient having lived much in a ditch. In this case they "escaped by dozens from the nose, ears, and eyes!!" Lastly, he mentions a case where one has been found in the brain producing madness,—a cause of madness in horses, says Raspail, well known to veterinary surgeons (p. 60).

In two of the above cases the patient may be literally said, like Herod, to have been eaten up with worms. I may here observe that Dr. Paris states in his "*Pharmacologia*" that formerly in Holland the punishment of death used to be inflicted by a *total* abstinence from salt,—*i. e.* not an atom of salt was allowed to enter even into the composition of the bread, or any other article of food, allowed to such culprits. The consequence, it is stated, was that death arose in such cases from the presence of

worms in all parts of the body, the criminal being literally devoured by them. Regarding this fact in its most common-place light, we may conclude that insects would far oftener deposit their ova in the living human subject were it not for the presence of salt — a *necessary* constituent of the blood and muscular fibre, and at the same time a powerful anthelmintic. As, however, a great excess of salt is as deleterious as its *total* absence, and many diseases probably act by diminishing its quantity,* *and thus favouring the developement of the ova of insects*, we have an indirect argument in favour of Raspail's views, viz. that it is advisable to employ camphor, aloes, and other anthelmintics (so to call them), as preventives of diseases. These same views would, however, also tend to make us employ *saline* purgatives a little oftener than Raspail suggests (phosphate or sulphate of soda), as these salts may often be in diminished quantity in the blood.

We now come to the section on pediculi (lice). Under this we have evidence from the following case (op. cit. p. 81) that these vermin can live in the intestines of the human subject, for a man having swallowed seven or nine, by the advice of a quack to cure jaundice, was seized soon after with violent appetite and marasmus. On opening his body a tumour full of them was found communicating with the intestines. Raspail conceives that the plica Polonica—a disease in which the hair is matted together in a disgusting way—arises from the ravages of lice, as they always appear to accompany it; also a cutaneous disease on the heads of children, sometimes

* In an analysis I made of human blood during the cholera I, like other chemists, found an almost total absence of salt—the chief cause of its very dark colour in such disease. This defect in the quantity of saline matter probably depended on the salts of the blood passing off with the copious *serous* dejections.

resembling impetigo, and sometimes rupia. A case is also given when the patient had lice boring out of all parts of his body; and then we have the well-known pedicular disease, where the patient is covered over with lice in the *subcutaneous* tissue, a disease which, singularly enough, seems as aristocratic as the gout, having killed Sylla, Philip II. of Spain, and some other great personages. He gives cases where tumours have been opened and found full of lice to shew that these subcutaneous lice do occasionally penetrate deeper into the body (p. 88).

From pp. 130 to 137 we have cases of convulsion produced by a caterpillar thrown up by vomiting; of disease of the liver, in which, after death, a caterpillar was found alive, having produced a schirrus of part of that organ; of vomitings produced by the presence of other anomalous worms (the larvæ of a black beetle); two other cases of vomiting from the presence of caterpillars; a case where violent pains in the head of a child arose from the presence of a species of caterpillar in the brain (the insect being found there after death), and apparently introduced there by the child smelling a rose.

Farther on we have cases where convulsions have arisen from the presence of leeches, introduced into the stomach by drinking copiously of impure water in the fields. But it is not till we have arrived at p. 143 (vol. ii.) that we come to the most interesting and important part of the work in a medical point of view, viz. on the presence of ascarides and other worms peculiar to living bodies.

In regard to the organ of generation of these worms, he observes, that it constitutes the greater part of the length of the worm (which length is ordinarily from a quarter to half an inch, like a piece of white thread).

He estimates the number of eggs in each worm at above 3000. (P. 147.)

He opposes the commonly received opinion that this worm lives *in* the excrements. He says it perishes when plunged there, or in warm or cold water. Being colourless, says he, it cannot live on the excrements, or it would gain something of their colour. It lives, says he, like a leech attached to the walls of the intestines, and is found only in the fæces in consequences of expulsion by anthelmintics. It is hermaphrodite, for he has never seen one without ovaries and eggs; but, nevertheless, accouplement is necessary for generation, the same worm at times acting the part of male or female. It is oviparous, and generally lays its eggs in the living body, at the surface to which it is attached; but in seasons of despair when expelled from the body, &c. in other places; and these eggs, which are almost an invisible powder, may, in such cases, mount from our utensils or linen, &c. &c. into the air, and thus again enter our system by respiration, and in this way be the *unknown* cause of almost all the diseases flesh is heir to, and which he enumerates at p. 156; this worm, he tells us farther on, being rather than any other the *ver ronge*ur of man.

He considers the itching of the nose as a sign of the presence of ascarides in the nasal fossæ oftener than in the rectum (at p. 167, he quotes authors who have found at least lombrici there). Redi, he says, found them in the lungs of one female hedgehog, and in the bronchiæ and trachea of two others, also in foxes, &c.

Bremser and Scharf have seen ascarides in the vagina a cause of nymphomania; Benedetti found them between the walls of the uterus and placenta in a woman who died pregnant; and Raspail himself has seen them in the genital organs of a child two years old, having got

there from the anus. Creeping up in this way, says he, they may sometimes cause *fleurs blanches*, and diseases of the uterus ; or if they pass into the peritoneum, dropsy or inflammation in the bowels, the cause of such diseases and numerous others remaining undiscovered ; for, be it remembered, says he, *they are not visible for some time after their escape from the egg*, and the original authors of the disease may have perished before the patient they killed.

Singularly enough, catalepsy itself has been traced to this cause by Dr. Kuhn in a child six years old ; for, after voiding 200 ascarides with his *urine*, the child recovered. (P. 171.)

It is all in favour of his theory, that the ova may occasionally be taken in by respiration, particularly in cases where the patient can never go to the water-closet, and, consequently, where a night-stool is used, that in such cases the worms, escaping from the *fæces*, may deposit their eggs on the stool, chair, bed-clothes, &c. Now, be it remembered, says he, *that such eggs may be taken up by the air, that they do not want the incubation of the mother to render them prolific, and also that they may remain so and survive her*. Here, then, says he, have we a new origin for an epidemic contagion, for typhus itself in its worst form. (P. 175.)

We have already observed that ascarides do not lay their eggs in the *fæces*, except in cases of despair. He was never able to discover the eggs there ; and he says (p. 176) that he has discovered them in his own expectoration, a confirmation, if it be true, of his theory, that they float about in the air, for this is more probable than that ascarides themselves were present in his lungs.

We now come to the second class of worms, the “*lombric*,” a worm a good deal larger than the one last described. On this subject Raspail thinks he has made

the discovery, that this worm deposits its ova often on the surface of the muscles; and that what Mr. Owen considered a new species and called *trichina spiralis* (having discovered numbers of them in dissecting the muscles of the arm), is really only a little "lombric," coiled up spirally on its sac or egg. The lombric, says he, propagates by eggs, but it does not lay them in the excrements: Owen's discovery has taught us one of the places where it deposits them. Redi says he had previously found the same little spiral worm in its egg on the abdominal muscles of an African lizard, and also in the lungs of a fox and other animals. (Pp. 180, 181.)

The bitterness of the bile generally stops these worms; but Raspail himself has seen one (p. 182) thrown out from the nose, having mounted up behind the palate from the stomach. It might just as easily, says he, have passed into the trachea or eustachian tube.

He pretty clearly makes out that these worms, in many cases, *actually perforate* the intestines. Leuwenhœk had always found them strongly attached to the intestines in *living* animals; and Dr. Leban met with a woman who had a tumour above the fallopian ligament, which opened of itself, and a worm of the size of the little finger "*et long de sept pouces*," came out, but nothing like fæcal matter followed. She found herself better, but it was not till three others had passed out of the same opening that the wound healed completely. Other cases of tumours in different parts from the same worms are quoted,* in which, like the above, *no fæces followed from the opening made by the worm*. Consequently, says he, *the intestine itself was sound previous to the worm's passage and the worm made the opening, as it did not arise from previous disease of the gut*. It must be confessed this

* See also six cases (p. 184) complicated with disease of the intestines.

is much better reasoning than we are accustomed to in medical works, and is as nearly proof as possible that these worms can actually perforate the LIVING intestines. Proceeding in the same line of argument, he shews how invagination of the intestines and consequent mortification and rejection by stool of such false or rather true membrane may occasionally arise from these worms sucking, *like a leech*, at two distant portions of the gut. From the same worms may hence, also, at times, arise vomiting of fæcal matter, and death from the *knotting* of the intestines.

Further on we have cases where these worms have actually been found in the lungs, womb, bladder, kidneys (of dogs as well as men), pericardium, heart, blood-vessels (in these latter causing false membranes, and consequent aneurisms), in the stomach (causing the "globus hystericus" from their rising into the throat), &c. &c. (Pp. 190-202).

In his first volume, p. 389, *et seq.*, he *proves* that the itch arises from the presence of a microscopic acarus, of which he has given figures; and he *thinks* small-pox, measles, scarlatina, and venereal diseases arise from an *animate* cause, the insect as yet undetermined.* In the present place (vol. ii. p. 206) he attempts to shew that the cutaneous diseases, leprosy and rupia, arise from the presence of the next genus of worms we are to consider, viz. the (filaria) "filaire;" a thin cylindrical worm of great length, and of which the greater number can live in water or humid earth.

* Vol. ii. pp. 312-326; also hydrophobia (*idem*, p. 347) from the presence of a worm, *lytta*, at the *frænum linguæ*. Ambrose Paré says garlic (or onions), with bread and wine, have, in his practice, cured this disease; so have strong vinegar vapours, or vinegar internally, infused on gentian, in Hervet's practice. Now these, says Raspail, are anthelmintics.

He thinks this worm, like the preceding, is introduced into the body by means of eggs taken in with our food or drink, or by respiration; for the skin never offers any sign of perforation until the worm perforates it to come out. He attempts to support this opinion, from having in a chicken which he dissected actually found the ova of this worm in the subcutaneous tissue (p. 208). However, as he could not produce the worm from these ova, and as the wound by which the worm gets into the skin may heal up, this point is open for future inquiry.

The presence of these worms seems common in the eyes of the Negresses in St. Domingo; and they sometimes pierce the conjunctiva to escape (p. 209). Of course they must sometimes produce blindness. In this country cases of worms in the eye have occasionally occurred, one of which will be found in the London Med. Chir. Transactions.

He conceives that the "filaire" is frequently taken in with the water we drink, and causes a long list of diseases, and even caries of the bones, as the Romans conceived (p. 213). There is a carious skull in Dr. Hunter's Museum at Glasgow, which certainly presents the appearance of having been occasioned by a small worm. "These worms," says he, "dry completely, and revive by moistening, as Fontana, Bauer, and himself have observed; hence they are well calculated to lie dormant in the bones, and continue their work at long intervals."

Under the head "Distome du Chien" (p. 220), worm of the dog, he says, that the ova are easily taken up by the air, and hence he cautions us against being *too intimate* with these faithful creatures. Flukes, so common in the liver of sheep, are also found, as he shews, sometimes in that of man.

We now come to the tape-worm (*tænia*), the giant

worm of the intestines, which has in different cases been the cause of hysteria, epilepsy, marasmus, and tetanus. Raspail thinks that the hydatid (a thin membranous contractile bag) is nothing else than the egg of the *tænia* (p. 232). If this theory be true, these eggs have been deposited in the lungs, stomach, liver, abdomen (giving the appearance of dropsy), bladder, uterus, brain, producing every variety of disease (pp. 238, 239). After the above enumeration of the various effects of acari and worms, he concludes that nine-tenths of our diseases are the work of such parasites (Resumé, p. 241).

Passing over his moral causes of diseases and his new classification, we come to the great revolution in the practice of medicine, to which his opinions of the causes of disease necessarily lead.

SECTION II.

RASPAIL'S NEW MODE OF TREATING DISEASES, FOUNDED ON THE ABOVE OPINIONS OF THEIR ORIGIN.

General Preventives.—Raspail, properly enough, begins by a reform *in the kitchen*, and would have us habitually use more condiments with our food. The cook should be told not to be so sparing of salt, pepper, garlic, onions in our sauces; of cloves, dried orange-peel, cannella, muscade, vanilla, angelica,* &c., in our pastry; these being the most useful species of anthelmintics, as well as the most agreeable, and particularly necessary

* He says somewhere in vol. i. that a man who lived at Montpelier, to the age of 120 and upwards, attributed his long life to his daily practice of chewing this root. At all events, it is proof that the root is harmless, and hence worth the *chances* of trial.

for the preservation of female health, as females do not smoke or drink so much generous wine, spirits, or *elixirs*, as men; all these being useful, as having more or less of an anthelmintic quality. As Raspail, however, would have ladies begin smoking cigarettes of camphor (inhaling the vapour of pieces of camphor put in a quill, glass, or ivory tube, and swallowing the saliva so impregnated), if they like to adopt this custom, they will have less necessity for the use of *other* anthelmintics. Raspail has smoked it himself both day and *night** for the last five years; and he finds that the only obvious effect is, that it suspends the appetite a little, like smoking tobacco (p. 372). I may observe, that as this night-smoking may be disagreeable, or perhaps, impracticable to some persons, a piece of camphor inclosed in a linen bag, and placed under the pillow, or tied so as to hang near the mouth and nostrils, would probably be amply sufficient to keep acari and all night vermin from getting into the nose, mouth, or ears, and depositing their ova there. Its power of diminishing hunger, but more particularly its remarkable power when sprinkled in powder over the nates of taking away all venereal appetite, as admitted by many old authors, and by Raspail himself (p. 442)—nay, even the very power itself of destroying insect *life*—shews that camphor is far from being inert, and consequently *kitchen* anthelmintics should first be resorted to for *habitual* use. It will be time to try the constant use of camphor, *as soon as any disease has obviously commenced*. Keeping a piece of camphor under the pillow, or near the person during *the night*, is all that in any case of perfectly good health I should be inclined to recommend as a constant practice. During *the day* no *visible* insects can get into the nose or mouth unde-

* See verses introductory to this Essay.

tected; and as to taking in the eggs of acari and ascari-
des by respiration, it seems better to run our chances of
this, trusting they will be destroyed by our kitchen an-
thelmintics, rather than submit to the possibility of in-
juring our constitution by constantly breathing camphor
vapour; for although Raspail has not *obviously* injured
himself in five years, still we know so little of the nature
of life that it is impossible to say he may not have
shortened it by this new kind of smoking: just in the
same way, in fact, as a man may live *many* years after a
copious salivation, and yet die sooner than if he had
never submitted to such excessive use of mercury. Be-
sides, men in various countries have lived to 100 or
above, without the assistance of camphor vapour; and
the experiment *is yet to make*, whether they can ever do
so with its *constant* assistance.

Raspail ascribes the power of camphor in destroying
priapism, just alluded to, to its power of preventing fer-
mentation (by converting the air in contact with it into
carbonic acid?). He has himself kept dead birds
(plunged in water in which was a piece of camphor)
from putrefaction for months together. We may here
observe that camphor, in common with many other
agents, has both the power of destroying fermenta-
tion—at all events, the putrefactive fermentation—
and destroying animalcular and insect life. Whether
Raspail's theory of camphor destroying priapism by de-
stroying seminal fermentation, and the life of the semi-
nal vermiculi, be true, experiment must decide; and also,
whether camphor placed in a room does not tend to in-
crease the amount of carbonic acid gas there. Another
point on this subject also requires further investigation,
before we can come to a correct theory on the mode of
action of camphor; it is no less than whether camphor
only drives acari and insects away by its *smell*, like

musk, lavender, &c. &c., or whether its vapours actually kill them. If the former mode of action be true, then we may conceive the habitual use of camphor to be far less injurious than if it actually kills. Raspail not only smokes it, but has also taken about fifteen grains a-day of the powder internally, for the last five years, without injury. He finds that four or five grains taken at night tend to promote sleep, though, of course, less powerfully than opium (p. 443). It tends to expel worms, as we see by the case, p. 444; but still it may only tend to make them leave their hold of the intestines, as they are generally *expelled living*. A few experiments would clearly be desirable, to ascertain which anthelmintics kill and which only tend to drive worms and insects away. Perfumes, lavender, musk, thyme, violet, rose, Raspail tells us, are anthelmintics, “au premier degré” (p. 378); and in the same place he says, that the use of musk about the person may be permitted to the lady to supersede that of camphor cigarettes, if she pleases; and that turpentine or tar vapour may be substituted for it by the poorer classes.

Now, it is almost certain that all these odoriferous substances have rather the power to drive away insects than to kill them; whereas tobacco (particularly in infusion) and mercury actually destroy them,—this latter killing the *pediculi pubis* in a very short time. If the venereal disease, and if caries of the bones, depends on an animate contagion, as Raspail supposes (though he admits the insect has not yet been found), certainly at first sight there would appear to be no better medicine than mercury for such affections; since this may be made actually to reach the interior of the bones, where we may presume it would not only *drive away*, but actually seize the worm in its work of devastation, and at once destroy it!

But on further reflection we shall see that the great objection to mercury, tobacco in infusion (for in smoke it is rendered comparatively harmless, by having been decomposed by fire), and the list of poisons is, that they cannot kill worms or insects without more or less injuring the vital powers of the patient. The same power that *kills* parasitic life must, in eight cases out of ten, tend itself to destroy the life preyed on. It is hence that we find that practice does not bear out the utility of mercury in caries, and that, in fact, some surgeons of extensive practice have properly rather attributed disease of the bones to the too copious use of mercury, than to the venereal disease itself,—a position, if it be true, which would shew that caries often takes place from an inanimate cause.

But, to conclude this digression, it may still be said, though mercury poisons lice, it may still not be able to poison acari, or some intestinal worms;* for we know that poisons to one form of life are not necessarily poisons to all others. This reflection brings us again to the position with which we started, viz. that experiments are wanting on this subject both as to odoriferous substances, and as to anthelmintics strictly so called.

I shall close these observations on camphor, by stating that Raspail makes his “Pommade Camphrée” by melting three parts of lard, and then putting one part of camphor powder in it, and stirring till the camphor is dissolved. Powder of camphor is easily prepared by dropping a little spirits of wine on camphor before pow-

* Redi, says Raspail (p. 525, note), considered mercury an anthelmintic; but as the men who work in quicksilver mines are much tormented with worms *at the anus*, Bremser seems to doubt its vermifuge powers. But, says Raspail, this is the best evidence of it, for the worms have been driven there by such power. The probability, therefore, seems, that mercury would destroy these as soon as lice, if in sufficient quantity.

dering it. Camphorated alcohol is made by leaving camphor in alcohol, and camphorated oil in the same way, only in this last case applying a *gentle* heat. He does not mention camphor water (made by leaving a piece of camphor in a bottle of water corked, until it tastes strongly of camphor). I would, however, recommend a glass to be drank night and morning, in cases where worms are suspected. The camphor in this case is well *dissolved*.*

*Eau Sédativ*e.—Take of liquid ammonia 1500 grains or drops, and put therein thirty grains or drops of alcohol saturated with camphor. Leave the mixture to combine one or two hours, and then add one “litre” (pint and quarter) of water, in which you have previously dissolved 450 grains of common salt. Put the mixture in a well-stopped bottle (glass stopper).

For delicate skins he only uses 80 instead of 100 grains of liquid ammonia. In the case of cerebral fever, and for applications on the cranium, he uses 130 grains.

	1st deg.	2d deg.	3d deg.
Salt water	8 parts	10 parts	16 parts
Camphorated ammonia .	1 —	1 —	1 —

In general he only uses the second degree.

To make the eau sédativ without the trouble of weighing, he recommends as follows:—Put salt in a tumbler of water *till the water can take up no more*. Put two little elixir glasses (petits verres) of liquid ammonia in a pint of water, then half a “petit verre” of camphorated alcohol; agitate, then mix with the tumbler of salt water; re-agitate, and cork well.

* Aloes may be taken at the same time. Camphor, as existing in camphor-water, seems far more likely to find its way into the torrent of the blood, and thus to destroy animate ova existing in any part of the body, than taken in lumps *undissolved*.

The employment of the eau sédative is by means of compresses dipped in it, taking care, if for the head, that none gets into the eyes. The hair, too, ought to be oiled to diminish its slightly caustic effect on the hair.

Mode of Action.—He thinks it is absorbed through the skin; and of course, *if it be*, it will have the effect of diminishing all viscosity in the blood, and rendering this more fluid. At all events, after extensive *practice* with it, he finds that cerebral fever soon disappears under its use, the pulse falling *sometimes from 180 to 80,** and the skin becoming fresh, though previously burning. As after *long* applications it becomes red, he at first only begins by lotioning with the hand. This eau is his great substitute for blistering, when used so as to produce redness of the skin; and for bleeding, when used so as to make the pulse fall. Like the Homœopathists he never bleeds; but, in opposition to them, he advocates the copious use of spices—of wine and spirits, too, in moderation.

Aloes.—He recommends four or five grains of the extract of aloes to be taken in the evening, and a bowl of broth after it (any other warm fluid will do as well), once a-week, even to persons in health. If in the morning by eight or nine o'clock it has not operated, the next week double the dose should be taken. It is, however, so difficult to conceal the taste of the aloes by taking it in this way, that persons would do well to try the powder made into pills with conserve of roses, or moistened crumb of bread, and then rolled in liquorice powder.

Aloes is Raspail's principal vermifuge, and by its action on the rectum it tends, no doubt, to drive ascarides even from this, their last place of refuge. It

* See his "Médecin de Soi-même," art. Eau Sédative.

also, in many of Raspail's patients, has caused a return of the menses after they have ceased a year. I may, however, observe, that in case of a tendency to piles persons should be cautious in the use of this medicine; the safest way in such case is not to take it all. I know that Raspail (p. 504), considering even piles to be caused by acari or some other parasite, recommends glysters of aloes in such diseases. But as he gives no cases, we may well for the present retain the old opinion as to their origin *in general*, and use only mild purgatives (castor oil, cream of tartar, and sulphur, &c.) which do not tend to irritate the rectum, and thus often, perhaps, cause fistula,—that most painful and disagreeable complaint.

Tar-water.—Put a little tar at the bottom of a decanter, and fill with water; let it remain till impregnated with tar. This water is an anthelmintic.

Decoctions or Infusions of Fern-root, and of Pomegranate-root.—These anthelmintics are made by boiling or infusing the above roots in water. He recommends a trial of these anthelmintics when camphor and aloes have, by repetition, lost some of their power. Fern-root, in doses from half an ounce to an ounce, may be taken in powder, two or three mornings in succession, in honey or treacle.

Assafœtida in glyster or pills.—These anthelmintics he recommends when there is reason to believe the worms have penetrated the flesh. Assafœtida, says he, is readily taken up, and penetrates all the tissues (p. 544, note).

Camphor, Cubebs, and Copavia.—Recommended especially when it is necessary to use anthelmintics that penetrate more especially the urinary and genital organs (note, p. 455).

Beet-root Powder.—I do not observe this to be mentioned in his large work, he probably not having tried

it when that work was written. He tells us (*Médecin de Soi-même*) his reason for trying it was, because infusion of beet-root is known to get to the bones and colour them; and he has never been able to find any worms or insects living on that root. Hence in its crude state he infers it is disagreeable, if not poisonous to them. On all these grounds it seemed well-suited for destroying the worm which he supposes is the cause of caries of the bones.

He says the infusion of the powder has the taste of liquorice (*réglisse*), but the exciting property of coffee. He makes it by infusing fifteen grains (one gramme) in half a pint of tar-water. He also puts a minute quantity of iodide of potassium to it. He has tried it in some cases of rickets and scrofulous caries with advantage; but says its use requires caution, as it may tend to render the bones less solid by combining with their earthy constituents. He recommends physicians to try it in caries. I should suggest, by itself, without tar or iodide of potassium; for this purpose fifteen grains of madder-root, dried at a heat below boiling-water, is to be put into a pint of water and boiled. A wine-glassful may be taken three times a-day for three days; and then left off for a week, or altogether.*

The above are not the only medicines he employs. On the contrary, on looking over his "*Succedanea*," he may be said *occasionally* to resort to most of the articles of the *Materia Medica*, except the salts of arsenic, lead, copper, silver, gold, antimony (except tartar emetic for a vomit in *case of poisoning*, p. 547), and mercury, except its submuriate, or calomel. He says, he admits calomel on account of its great insolubility; but I may oppose to this, that insolubility does not preserve from the con-

* *Méd. de Soi-même*, p. 106.

stitutional effects of mercury, as we know by its power to salivate,* and as might reasonably have been inferred from the following experiment which I made on a cat many years ago with *sulphat* of lead, conceiving from the statements of Orfila and others that the insolubility of this substance would have prevented constitutional effects. I found, however, after the cat had been made to eat bread and milk with which sulphat of lead was mixed for some days, that a decided palsy of the limbs took place to my dissatisfaction, and the poor animal, in a state of half-delirium, committed a sort of suicide by jumping out of an open window. After such experiment, I cannot have faith in sulphat of soda, which is still recommended in medical works, as an antidote for poison by the salts of lead, on account of its forming an *insoluble* sulphat. This may, perhaps, be one step better than leaving the soluble salt; but a far better step is to get it all out of the body as soon as possible.

It seems to me, therefore, that the wisest plan is not to rely on *insolubility*, but to exclude mercury in all its forms from medical practice, except, perhaps, in some peculiar cases of venereal disease, where the patient's occupations prevent him from giving the non-mercurial practice a fair trial. Neither should I employ tartarised antimony as an emetic until ipecacuanha and all other vegetable emetics *had failed*. This is Raspail's general line of practice, as he tells us, with regard to prussic

* Besides, when he has so many vermifuges, it is contrary to his own principles to use a noxious mineral agent, *until* all other vegetable worm medicines have failed. He says he has given calomel to the amount of *fifteen grains even to infants*, without injury. As he admits, however, it is apt often to be very injurious in consequence of bad preparation, it seems far better never to use it until *all* other substitutes have failed, and even then never in such large doses. (Méd. Soi-même, pp. 66, 67.) From one to two grains in sugar is the common dose for infants.

acid, belladonna, hemlock, strychnine, stramonium, &c., all of which are likewise totally banished from his practice; opium alone being permitted when *camphor is not powerful enough to produce sleep in exhausted subjects*. In this line of practice I need not say I agree with him, except, perhaps, in *desperate* cases, to allow the vegetable poisons in homœopathic doses; the necessity for which I leave to be decided by further experience. Raspail admits the iodide of potassium, but says in his small work, he doubts if it has done any good in his practice,—a sufficient reason for discontinuing it (except as it may be found in mineral waters), knowing that it has often done a great deal of harm by causing absorption of the *healthy* glandular tissues, when given as a remedy for goître.

He admits salts of iron, sulphat and phosphat of soda; and thus, in fact, may be said, without being aware of it, to adopt a line of medication similar *in its general features* to the one already proposed by me, with the exception of his theory of disease, and his consequent recommendation of vermifuge medicines, which he still, be it observed, draws almost entirely—and with justice—from the vegetable kingdom. On this subject he has brought me over in *many respects* to his opinions, as I shall state a little more at length when I have selected a few *cases* from his work, as proofs of the correctness of his views.

Cases (Accidents).—The eau sédative applied ten days on a tumour as large as the fist on the ankle, arising from a kick from a horse, cured it completely.

In a case in which all the internal surface of the hand had been laid bare, camphor powder was sprinkled over the part; *no suppuration followed*, and the next day the excoriated surface was dry and gradually healed (p. 405).

Observations.—In the first case the eau sédative sup-

plied the place of leeches, the regular routine practice in such cases ; in other words, inflammation was cured by a violent stimulant, for such is the eau sédative, notwithstanding the name he has given it. In the second case a stimulant also prevented suppuration, and thus shortened the time of cure considerably.

Burns.—Camphor powder is to be sprinkled over the wound, and covered over with lint on which the camphor ointment is spread, so as not to let the wound be exposed to the air. The eau sédative is to be applied *around* the part where the wound is. In this way, he says, he cured a child whose face was a mask of scars, the eyelids being glued together.

Apoplexy.—The head and neck to be covered over with cloths wet with eau sédative. Spine to be well rubbed with camphor ointment, subsequently a purgative clyster. Aloes to be given by the mouth as soon as signs of life, and the patient to be put in a bath in which, after it is half full of water, four ounces of alcohol saturated with camphor, and one and a half pound of common salt, are to be put and stirred with a red-hot iron rod, which bath he calls his alkino-ferruginous bath. Patients have recovered in a quarter of an hour by this treatment,—a remarkable case of which he gives at length in his small work (p. 136). He has also seen palsy cured in a few minutes by the same plan. He also gives cases where fish have been restored, almost like a miracle, by the eau sédative (*Méd. de Soi-même*, p. 137).

Violent Pains in the Head.—In p. 483 of his large work he gives his own case, in which intense pains of the head continued many days, and at last caused loss of sight, apparently from the pain the light gave. It occurred to him to make the lotion which he calls eau sédative, and which we have seen in the first case he uses

instead of leeches, and in the last (apoplexy) instead of bleeding. This was the first time of trying it, and, he says, scarcely a few minutes passed before he felt better. He used it a month at intervals, when he was quite restored; his sight only remaining permanently affected, being now obliged to use convex glasses to read.

He says the eau sédative penetrated *through the bones of the cranium*, dissolved the congestions, killed the larvæ or worms (which he supposes to have been the cause of this violent attack), and likewise dissolved them (p. 486, end).

Observations.—No doubt if ammonia and common salt, the base of his eau sédative, find their way through the *bones of the cranium*, they will at least liquefy the blood. But I think it *very* doubtful whether they can do so,—at all events in *sufficient quantity*; for by experiments *out* of the body, I know that it wants a considerable quantity of ammonia to break down a clot, and no doubt more to dissolve worms. I hence have little hesitation in concluding that his *theory* of the mode of action of the eau sédative is erroneous. It probably resolves congestions by stimulating the vessels, and forcing their contents onwards *still undissolved*. It is also *probable* that his headach did not arise from worms; but as he tells us in another part of his work that he has voided ascarides, of course I would not venture to say decisively that these had not made their way even into his brain. But, notwithstanding these opinions, I perfectly agree in the propriety of his mode of treatment, and am ready to acknowledge the benefit he has done to medicine, by enabling us to treat local inflammations, apoplexy, &c., without leeches and bleeding, which, by weakening the patient, always lengthen the convalescence.

Asthma.—He thinks this often arises from the ova of

ascarides deposited in the bronchial tubes. He quotes the case of Madame Simon (aged 60), who had an attack every winter that threatened her life; and also the case of a man (Clamart). Both these and many other cases were cured by the use of the cigarette of camphor; frictions with camphorated alcohol on the throat and lungs; internal use of aloes by mouth and injection; aromatic nourishment, *i.e.* highly-seasoned meats; and gargles of salt-water. If the camphor cigarette be not active enough, a little camphorated alcohol is to be put on a cloth, and held to the lips during the paroxysms (*Méd. Soi-même*, p. 139). See also large work, vol. ii. p. 454, for two other cases greatly benefitted by similar treatment.

Diarrhœa: Dysentery.—He quotes the case of a man, who had been almost given up by the doctors, labouring under the former of these diseases, who was cured in a fortnight by frictions with camphor ointment five times a-day and two or three times a-night, camphor cigarette, alcohol on the region of the liver, and highly-seasoned diet. When this man came to Raspail, he had been so reduced by the antiphlogistic diet that he could not get out of his arm-chair without assistance, and did not expect to live (vol. ii. p. 492).

Dysentery, like diarrhœa, often depending on the presence of acari or worms, requires the same treatment; except that where *acid* fruits have been taken, the internal use of two or three doses of carbonate of soda is recommended.

Fevers, Typhus, &c.—Raspail considers typhus fever especially as arising from an animate cause, though, in this case, he gives no instances where he has actually found acari or worms in the alimentary canal. The success of his anthelmintic practice in this disease would appear to bear him out in his conclusions, as we shall

presently state ; but it is to be hoped that physicians will hereafter attempt to discover this animate cause, if such it be.

A girl of fourteen, with burning pain in the head, skin darkish, and *who had lost the power of understanding any question put to her*, was treated by eau sédative to the head and neck ; camphor ointment, frictions, and camphor, held to or put near the mouth ; aloes internally, spiced diet, &c. ; and in three weeks she was quite well, but altogether unable to remember that any questions had been put to her at the time stated above.

A young woman was found by her mistress on the ground in a state of insensibility, voiding both upwards and downwards black matters, and in a state of burning fever. Treatment same as the above, and the *next morning* she was able to walk, and, in fact, cured.

Two other cases of a similar character are to be found at pp. 207, 208 of Méd. Soi-même, along with these.

Many years ago I myself caught the typhus fever from some hospital patient. Coming on as usual with shiverings, it continued its course till I became delirious. My chief or only medicine after a purgative was a glass occasionally of port wine. Under this treatment the fever gradually subsided, but left an abscess behind, which in the course of time was also cured. The great debility I felt was the cause of my taking to wine instead of the antiphlogistic treatment ; and in this respect I may be said to have adopted Raspail's plan, without having his or any other particular theory of the disease in view. Although I should not exactly recommend wine so early in the disease, as this might have been concerned in causing the abscess, still such treatment was probably better than adopting the bleeding system,

which was general with hospital patients at the time. Raspail's intermediate course is better.

Fleurs Blanches (Leucorrhœa).—He considers this disease also to arise from an animate cause. He says that camphor powder or ointment introduced into the vagina, with aloes, &c., occasionally, "triumphs infallibly" over the disease.

Ophthalmia (Inflammation of the Eyes).—He puts this even down commonly to an animate cause. "The inflammation of the conjunctiva," says he, "does not last three days, if *very fine* camphor powder is blown on it two or three times on different occasions." The operation gives a little pain, and produces a flow of tears. In the other affections of the eyes (amaurosis, &c.), he uses camphor ointment on the eyelids, eau sédative on the temples, and camphor powder as snuff (vol. ii. p. 509).

Observations.—We here see that he applies his stimulating system with success, even in inflammation of the eyes. It is singular that, according to Clot Bey and other authors, the stimulating system—viz. a strong solution of sulphate of zinc and alum—succeeds best in the Egyptian ophthalmia, and that leeches and the antiphlogistic treatment fail (Clot Bey's Egypt). This may give us confidence in Raspail's treatment of inflammation of this delicate organ. Nevertheless, common inflammation of the conjunctiva gets well *in this country* by the antiphlogistic treatment, as all physicians know.

Itch and other Cutaneous Diseases.—It is singular in his account of the itch, he does not mention sulphur ointment, which, nevertheless, as physicians well know, approaches almost to the nature of a specific. He recommends camphor ointment, &c., which would probably also cure it; but he gives no cases. He gives,

however, a bad case of impetigo cured by the application of camphorated alcohol, &c., and one of prurigo by camphorated ointment. He recommends much the same treatment for all cutaneous diseases, conceiving them all of animate origin, though he has only discovered the insect in one of them, viz. the itch.

Venereal Diseases.—These he considers also to be of animate origin, but does not consider that Dr. Donné has discovered the acarus causing them. He considers the mercurial practice whether internal or external poisonous and injurious even in the mildest form, such as only slightly affecting the gums. No doubt the disease can be cured without this ; but still many who have had large experience conceive that where the patient is *not entirely his own master* a *very mild* mercurial course is advisable. It is certainly desirable that the use of mercury in this complaint were done away with altogether. Nevertheless, it must be confessed Raspail has not given enough cases to make us rely *at present* with perfect confidence on his treatment. Only two are given of a mild character. His plan of treatment is to take a little camphor three times a-day in a wine-glass of decoction of sarsaparilla, to a pint of which fifteen grains of iodide of potassium have been added ; pommade, or powder of camphor, to all ulcerated parts, or *écoulements* ; tar-water in all drinks, and injection of the same, or of camphorated oil in *écoulements* ; camphor cigarette, and well-seasoned diet, with generous wine.—*Méd. Soi-même*, p. 218.

In his large work he gives experiments which tend to shew that the disease cannot be communicated by sexual intercourse, when camphor powder is used *immediately after* impure connexion as a preventive of contagion. Before the camphor was applied the parts were well washed (vol. ii. p. 520). These experiments probably lead him to conceive that camphor has also a curative

action. It is, however, to be observed that he uses sarsaparilla and iodide of potassium largely, both of which (according to many authors) pretend almost to specific claims in the cure of syphilis. Certainly they are far preferable to mercury, supposing their claims well grounded. Raspail, however, much modifies this treatment of venereal diseases by using at the same time *spiced diet and good wine*, a plan condemned by Dr. Thomson and the non-mercurialists generally. It is to be hoped Raspail's treatment will meet with that practice on a large scale, which can alone enable us to pronounce an opinion on its merits.

White Swellings and Mortification of old Age.—In p. 237 of his small work a very bad case of white swelling is given, cured by compresses of eau sédative and the use of the camphor ointment; and at p. 244 a case of a *mortified* ulcer of the leg in a man aged eighty-three, which soon got well under the camphor treatment.

Whitlow.—At p. 225 two cases are given where camphorated alcohol, &c. succeeded. In one of these cases black spots like gangrene had appeared.

I here close my account of Raspail's treatment of disease. As he treats almost all other diseases on the same principles, but gives no other *striking cases of cure* than those above mentioned, I have thought it unnecessary to make any further selections from his works, since it is in every body's power to try the camphor treatment in any disease when once the *general plan* of proceeding is known.

REMARKS
ON
THE EXAMPLES OF LONGEVITY

BROUGHT FORWARD

BY SIR J. SINCLAIR ;

HIS OBSERVATIONS ON TRAINING ; JERNITZ'S RECIPE FOR
THE ELIXIR OF LONGEVITY ;

AUTHOR'S RECIPES FOR INDIGESTION, COUGH,
PILES, TOOTHACHE, AND FOR PURGATIVES ;
ON BLEEDING, GOUT, DIABETES, VENEREAL DISEASES,
AND THE MEANS OF THEIR PREVENTION, FROM
PERSONAL OBSERVATION ON CASES,

&c. &c. &c.

REMARKS,

§c. §c.

SIR J. SINCLAIR, in his *Code of Health and Longevity* (edit. 1818), has given brief notices of seven or eight individuals who have lived to 112 years, and upwards. Zortan, a Dutchman, the oldest of these, lived, it is said, to 185! In our own country, Jenkins lived to 169, and Parr to 152. It is to be regretted that Sir John could not obtain any information of importance as to the mode of life of these individuals, except that Parr's maxims were, "Keep the feet warm, and the head cool; rise early, and go to bed soon; be moderate in diet, and sleep if you are inclined to get fat" (p. 61). Probably Parr himself did not attempt to diminish his quantity of sleep, since he seems only to have recommended this step to those inclined to get fat.

In my remarks on Cornaro, I have considered the propriety of attempting to diminish the quantity of sleep that each individual is inclined to take, very doubtful; and I find Sir J. Sinclair says that those undergoing the training discipline are recommended to go to bed at ten o'clock, and to sleep about eight hours. He adds, "Eight hours' sleep is generally reckoned necessary,

though much depends on habit. The ancient athletæ were permitted to sleep as many hours as they chose" (p. 36, Appendix). Very little liquid was allowed these athletæ, and the same is the case with the modern system of training, the quantity of liquid taken *during the whole day* being recommended not to exceed three pints (p. 33). The guides in Switzerland also always recommend travellers to take little fluid ; being of the same opinion as the trainers, that drinking increases transpiration, and all transpiration which is not the result *solely* of exercise is weakening (p. 34).

Something, however, must depend on habit in this case, for I found I could get through more work up the mountains by rather plentiful draughts, in proportion as the transpiration was great ; and Sir John gives a case where a gentleman resolved to adopt this system of drinking scarcely any thing, and that only wine, who died very shortly afterwards from the effects of inflammation (p. 56). The tendency to this must obviously be increased by this *dry* diet ; and hence, probably, the chances to be cut off prematurely ; though I have no doubt that the system is good, when, as in training, the object is to acquire a temporary increase of strength.

It is remarkable also that the trainers begin by an emetic and an ounce of Glauber's salts taken every two days for six or eight days. The ancient athletæ also adopted emetics and glysters at the commencement. Game-cocks and race-horses are also purged previous to being put on their regimen.

Very little salt or spices are allowed to trainers ; but vinegar is allowed, as it tends to render less fluids necessary (salt and spices having the opposite property), and also promotes leanness. Malt liquors, also wine and water, are allowed, but no spirits ; tea, like all fluids, should be taken nearly cold ; under-done mutton and

beef are preferred among solids; and pies, vegetables, and cheese are avoided. *Stale* bread is used.

Such is the general outline of the system of diet found the most effectual for raising animal strength to the highest pitch; and, by adopting the system only *partially*, persons of a weak constitution will no doubt be able to add to their strength. Indeed, it would not be desirable to follow it out completely for more than two or three months between the ages of eighteen and twenty; for the probability seems that if training were kept up *very* long, it would rather tend to shorten life by predisposing to inflammatory complaints (as already observed), and also on the principle that all long-continued and violent excitement tends *sooner* or *later* to produce a degree of debility and also to dispose the person to excesses. Thus Galen says the ancient *athletæ* rarely preserved their strength so as to be able to appear in public longer than five years, and that they were generally short-lived; partly owing, no doubt, to the excesses they generally run into when out of training (p. 40). But, as Sir J. Sinclair says, many boxers have lived to upwards of eighty, and that training does not wear out race-horses or game-cocks, we may agree with him that it would be advisable that most men should go through *one* system of training regimen for a month or two; and this seems to leave the body stronger than before the training began, at least for some years.

“The union of vigorous exercise and pure air is the great secret for the acquisition of strength. *Diet itself is but of secondary consideration*” (p. 37). Hence, continues Sir John, the inhabitants of the Sandwich Isles, “*who use no stimulating food or drink*,” were found stronger than many British seamen. Under such circumstances it would seem advisable to try the training system, using the same amount of exercise in pure air,

but without so much stimulating food or so much malt liquor. The ancient *athletæ* took water or sweet wine, but water *alone* is never allowed to modern trainers (p. 33).

We have seen that Raspail recommends a dose of aloes occasionally; and, as evidence that aloes cannot be injurious to the constitution (at least where piles do not exist), we may here give the general outline of the receipt for Dr. Jernitz's elixir of longevity, by taking from seven to nine drops of which daily, Jernitz himself lived to 104, his son to 100, and the whole of his family to a great age (Op. cit. p. 241).

An ounce and a drachm of powdered aloes; one drachm of powdered zedoria, and of powdered or bruised gentian-root; ditto of saffron; ditto of rhubarb, and of ditto of the theriaque of Venice. Pour a pint of brandy on these, and leave it well stopped for nine days, shaking frequently. The brandy may now be poured off, and put in a glass-stopped bottle. Another pint of brandy should now be put on the mass remaining at the bottom, and left a similar time. It may then be mixed with the former, and put aside for use. The liquors, of course, in each case are to be quite clear before they are poured off, and to be kept in good ground-glass stopper-bottles.

It is remarkable that aloes form by far the greatest portion of these ingredients, and so far it resembles Raspail's system; having an advantage, probably, over that by using also gentian, another bitter, the peculiar action of which is to strengthen the digestive powers. Hence Jernitz recommends it to be taken for indigestion. Dose for this, two tea-spoonsful in four of tea; for colic, two ditto, in four of brandy; for fits of gout (the fit approaching), three spoonsful, quite pure; for worms (see Raspail), one ditto before eating, for eight days; for

dropsy, one ditto, in white wine, for a month ; for ague, one ditto pure, before the cold fit ; and if the fever, says he, is not cured by the first or second dose, it will undoubtedly be so by the third. For sickness at the stomach, one spoonful quite pure is to be taken. It should not be taken after milk or salad.

I have thought it as well to copy this receipt, as some persons may wish to try it daily. I, however, should not be inclined to recommend more than its *occasional* use. Longevity is evidently hereditary ; so that Jernitz's son attaining the age of 100, may *partly, at least*, be accounted for by his father having lived to 104.

I shall now add some other receipts which I have found useful in my own experience, referring the reader who may wish for information on that most important of fluids, the blood, to my work on its properties in health, and in the diseases, scurvy, cholera, &c. published at Highley's, 32 Fleet Street.

For Indigestion.—Compound infusion of gentian, an ounce ; tincture ditto, a drachm ; carbonate of soda, ten grains. This quantity (*i.e.* two tablespoonsful) to be taken twice a-day, two hours before eating. As it keeps well, eight ounces of the mixture may be made up. This mixture is also indicated along with aloes, camphor, &c. suggested by Raspail in cases of worms. (See our Introduction to Raspail's Essay, pp. 32, 34.)

Cough Mixture.—Water of acetate of ammonia, three drachms ; wine of ipecacuanha, ten drops ; syrup of white poppies, half a drachm ; water, one ounce. Have eight ounces made up, and take two tablespoonsful whenever the cough is troublesome. I have frequently prescribed this, and found it often gradually succeed in removing a fixed pain in the side accompanied by cough.

Medicine for Piles.—I find Dr. Peschier, of Geneva, in a pamphlet which he gave me while there, cured a *very* bad case of these merely by rest, low diet, and camomile in homœopathic doses. I state facts, without presuming to decide whether the camomile acted on the disease or not. I have known them yield to rest, cream of tartar, and sulphur powders, and ointment of powdered nut-galls and lard, for which Raspail seems to substitute camphor-powder and lard, or camphorated alcohol,—a plan that may be tried last, as a painful one.

Toothache.—If a black spot has *only just made* its appearance, the best way is to get it filed out; if there be a hole in the tooth, stop it with camphor, or lint dipped in camphorated alcohol or tincture of opium. If the pain still continues, get the gum well lanced, or you may do it yourself with a sharp penknife, and occasionally rub the gum and take in a mouthful of warm water to encourage the bleeding. This will save many a tooth, which people are apt to get drawn much too soon. Only a few days ago I stopped a most excruciating toothache of two days' standing by lancing the gum.

Purgatives.—Morison's pills are generally considered to be composed chiefly of gamboge and aloes; but from the watery nature of the evacuations they produce the author would consider that elaterium entered also into their composition. He has himself tried them (No. 2) in a *full* dose, in which case they produced evacuations very similar to what would have occurred had Epsom salts been taken, except that they acted more especially on the rectum; indeed, too much so to be taken with safety where piles or a tendency thereto existed. In subsequent trials he has taken them in half the dose recommended by Morison, in which case also they pro-

duce watery evacuations without that violent and sudden action which, except in cases of great inflammation and fever, is not desirable. Having tried, also, frequently the compound gamboge pills, the colocynth, and compound rhubarb pills of the pharmacopœias, and also powdered aloes (recommended by Raspail), the author can confidently assert, that this tendency to produce *watery* evacuations is peculiar to Morison's pills, and renders them a useful medicine in inflammations, or derangement of the health, occurring so frequently in consequence of the accumulation of hardened fæces in the rectum. The author's peculiar views lead him to prefer Seidlitz powders, Epsom or Glauber's salts; but where people object to these from nausea, &c., or where, as is sometimes the case, they are inefficient completely to unload the bowels, half the usual dose of Morison's pills (No. 2), *at least to begin with*, taken *just before* going to bed, will be found an useful substitute. As another peculiarity of these pills is their *very quick* action, it should be borne in mind that they are best taken as late as possible *if taken at night*. Where, on an emergency, a full dose is required, they may be taken *in the morning*, and will be found to operate in two or three hours—a great advantage this in some cases of inflammation or oppression of the rectum from hardened fæces.

Two of the compound gamboge pills at night and half an ounce of Epsom or Glauber's salts the following morning, with half an ounce of infusion of senna, will operate very powerfully and produce very liquid evacuations, particularly if a *large quantity* of warm *weak* tea be drank immediately after taking the salts—a proceeding that should always be adopted when saline medicines are taken. The patient also should abstain from eating for at least two or three hours after having taken the salts and senna.

Gamboge pills, salts and senna, taken as above, will produce much the same effects as Morison's pills, with the exception of not acting so much on the rectum. They will, therefore, I conceive, have an advantage over Morison's pills where piles exist. But where these do not exist, Morison's pills possess the advantage of producing liquid evacuations—of washing out, as it were, the alimentary canal—without our being obliged to *taste physic*. I apprehend where an anthelmintic is wanted a patient would do as well to take three of Morison's pills (No. 2), as ten grains of aloes recommended by Raspail; and as I am of Raspail's opinion that some precautions should be taken against worms, I would certainly recommend a dose either of one or the other occasionally. Jalap and scammony may also be used, if preferred. Where the object is completely to unload the bowels they will much assist aloes, both as to general action and also its action on the rectum, which by itself alone is often but slight.

I have been induced to incorporate the above remarks on Morison's pills in the present work,—1st, Because I have tried their effects on myself and found a beneficial peculiarity in them, which may exist indeed in other pills, patented or not, but of which I cannot speak from personal experience; 2d, Because Morison has repeatedly stated on his oath that they contain no mineral admixture; and, lastly and chiefly, Because Morison has been (as far as I know) *the first man in this country to object to the exhibition of poisons in medicine*. I am not certain whether I derived my ideas on this point from his notices in the *Hygeist* or not. I believe they occurred to me quite independent of him, and certainly of Raspail, for the present Essay on Cornaro was chiefly written at Venice before I knew any thing of Raspail's views. Nevertheless, I most firmly agree with both on

this most important point, and of course admit their priority of publication, and I shall not hesitate to confess that I think Morison entitled to the thanks of the public for the objections he has printed against the use of poisons in medicine. Without professing to be a man of science, which he was not, he had sufficient common sense to see the uncertainty of medicine and sufficient moral courage to bear the opprobrium of "quack," or any other appellation. I am not going to attempt to set him forth as a martyr in a public cause, for he worked, and probably properly, for his own interest. But, motives apart, I do not hesitate to say, that if I were compelled to choose between a physician who is in the habit of giving at least any mineral poisons and trusting to Morison's pills for constitutional disease, I would not hesitate to give the preference to the latter; and if I ever consented to take mineral poisons it would only be when Raspail's plan, mesmerism, hydropathy, homœopathy, and every other had failed. But I do not think that the misery and misfortune of even the most malignant disease would ever induce me, *EVEN THEN*, to give it a trial, for it is a system so contrary to all sound views on physiology. I am not going to contradict the testimony of many medical men as to the disappearance occasionally of diseases during the administration of these poisons; but what I say is, Have you seen your patient six or seven years afterwards? is he not of an unhealthy aspect? and has not the poison sown the seeds of *another* chronic disease? As Alexandre Dumas says, "Water your cabbage with a *weak* solution of arsenic, and let an animal eat your cabbage; give then the animal to your friend to eat, and you will not the less poison him because you have done it by an *infinitesimal* dose, because by this means (the ancient method of *slow* poisoning) you have produced a chronic disease, the origin of

which even the wisest of the faculty could not have divined." There are enough chronic diseases without our adding to their list by the administration of poisonous minerals.

So much for purgative *medicines*; but the best *habitual* aperients are brown bread, fruits, greens, and moist sugar in tea or coffee. Brown bread itself is generally sufficient. It is better to try this than resort to purgatives, except occasionally when the presence of worms is suspected.

Bleeding.—We have already seen that Raspail, by his *eau sédative*, dispenses with the use of the lancet. Neither the homœopathists nor hydropathists bleed. Dr. Dickson, in his amusing work, the "Fallacies of the Faculty" (1845, pp. 14, 194), gives a table to shew that out of a certain number of cases more recovered who were not bled than who were bled, *even in apoplexy*. The late Sir G. Lefevre also considered, and no doubt properly, "that large bleedings tended to produce dropsy and chronic affections." To give Morison his due on this point too, it must be admitted that he seems always to have opposed bleeding; and Mr. Tothill, member of the Royal College of Surgeons (who practises on Morison's plan), gives many cases of acute inflammations cured without bleeding. At the same time Dr. Dickson was, however, probably the first who shewed the advantage of the non-bleeding system by tables,—*i. e.* in a scientific way; and it is to be regretted he still prescribes poisons.

Gout.—I was acquainted with a country surgeon in large practice who, when an attack of gout seemed approaching, used to take a *warm* stomachic medicine, such as the recipe for indigestion above given, only infusing bruised cloves at the same time with the gentian. This mixture, attention to diet, and warmth to the part, was

his remedy,—certainly a more harmless one than eau médicinale, or colchicum. The hydropathic system and mesmerism have also benefitted gout, so here is a choice of treatment. In fact, in this case, as in most others, we work in the dark, and it is best to try simple means first, as they sometimes cure *unexpectedly*. Clot Bey says gout, consumption, and hydrophobia, are unknown in Egypt; and as it is chiefly men of fortune whom the gout attacks, two years in Egypt might very properly be recommended to them after ordinary means had failed. The same remark applies to *incipient* consumption (for in any other stage the suggestion is useless). For this complaint I have a patient out there now.

Diabetes.—I have known a bad case of this disease to be cured principally by confinement to meat and stale bread for food, the latter in as small quantity as possible, with sherry and water as drink. Many cases are on record of its having also been cured by living some time in a very hot climate, which acts by exciting the action of the skin.

Venereal Disease.—It is difficult to get this complaint out of the system when once in; many secondary symptoms, which withstand the routine practice of decoction of sarsaparilla, are benefitted by the cold-water cure, *i.e.* by the *sweating* part of this plan.

I shall here make a few remarks on the *prevention* of venereal diseases. We have seen that Raspail recommends *finely* powdered camphor, and states that, by the use of this, a party he knew voluntarily exposed himself to many bad cases of infection without contracting the disease. It would be as well, however, to use the camphor in a less dry state, as in certain constitutions it might cause a degree of inflammation, as it always produces more or less of a burning sensation when applied to mucous surfaces. This property of camphor, there-

fore, should be diminished by the admixture of a little water or solution in oil. Without presuming to deny the efficacy of camphor, I may here state, that while at Naples some years ago, I mixed some *very fresh* gonorrhœal matter with solution of chloride of lime, and found it to render the same of a brownish hue—in fact, to decompose it. There seemed little danger, under such circumstances, of rubbing this on the glans penis and about the entrance of the urethra; which was accordingly done, and without *any* symptoms of venereal disease subsequently appearing. Chloride of soda was found, in a French physician's (Coster's) experiments, to succeed equally well; and not to produce the unpleasant astringent effect which followed the use of chloride of lime. I have known a gentleman employ the chloride of soda, mixed with about half its quantity of water, for years without contracting any venereal disease, though he must, doubtless, often have exposed himself to such risk. To be certain, however, of no evil consequences following, he always used it both before and after connexion—a necessary precaution, no doubt—as well as mixing it with a little water at first, since if too strong it may tend to cause inflammation. As the chloride has been tried by the French physician in other cases of infection—in plague, itch, &c.—and found always to destroy the infecting power, I do not hesitate to recommend this in preference to camphor, as having, I think, stood a better trial. It has not only prevented disease for years, but its use during the same period has done no injury to the person using it—an important consideration. In order to lose none of its power, it should be kept from the light, and in a well-ground glass-stopper bottle.

There are two particular reasons why I thought it as well to allude to the prevention of this disease: 1. Be-

cause a lotion for this purpose is sold at an exorbitant rate at one of the largest patent medicine warehouses in London, the utility of which lotion I should consider VERY doubtful ; and, 2. Because gonorrhœa, or a very similar disease, may sometimes be produced in men by leucorrhœa, menorrhagia, or other *non-venereal* diseases in women ; so that, in fact, an ignorant man might in such case be led to suspect his wife unjustly. Let him, under such circumstances, then, use the solution of chloride of soda as a preventive of infection.

Solution of chlorine holding mercury in solution has been sold in Paris for the object in question, but neither this nor chlorine alone are at all to be compared with solution of chloride of soda, as this, from its harmless, non-astringent nature, may be used *before* as well as after connexion, which is not the case so well with the above, *but which is absolutely necessary to ensure against contagion* ; at all events in *very* susceptible subjects.

I have in a former part of this work stated my aversion to the use of mercury in all its shapes, and if asked how I would supply its place, I answer by extract of taraxacum or dandelion. This acts on the liver, and promotes the secretion of bile ; and if it does not succeed in certain cases, horse exercise, or, if objected to, jumping, will be found of certain operation,—at least, in most cases in which medicine can avail. Hence it never can be justifiable to use mercury in any disease, except, perhaps, sometimes in primary syphilis, when the patient objects to the tediousness or discipline required by other modes of cure. I still hope to see mercury banished from rational medicine, even in these *rare* cases, in a year or two, by further experiments on the non-mercurial treatment of primary syphilis.

Enough has been said above to shew my bad opinion of that medical practice in which the use of metallic

poisons is allowed; it is almost superfluous to state, therefore, that I would wish to have them excluded as rigorously from our food and drink. For this purpose, it would not be useless to have a sort of "*travelling*" chemical professor, whose office should be to analyse the water of our public companies, and see that it was free from lead and even zinc in this *iron-zincing age*. An Edinburgh professor some time ago shewed that water *in particular circumstances* takes up an appreciable portion of lead. If, then, our travelling professor were not allowed to journey into people's houses to inspect the cisterns and kitchen utensils, he certainly ought to be allowed to see that water enters there free from mineral poisons. Dr. Girtanner, only a few years ago, very properly expressed his belief that life was at present shortened in consequence of our being obliged to take in more or less lead, copper, and tin, with our food and drink.* What, then, will be said when we also take these poisons *wholesale*, by way of physic? Let us hope from this time forth we shall hear no more of lead, antimony, tin, bismuth, zinc, arsenic, or scarcely even mercury, as *medicines*, indeed; as to gold, silver, and copper, they are evidently much more in place, and no doubt far *more useful*, in the pocket than in the body. Talk about medical reforms in charters and increased exclusiveness: let us begin at the beginning. *The Pharmacopœia wants its vomit as well as its purgative*; and until it is thoroughly cleansed of all its extraneous metallic matters except iron (which Nature herself admits there, since she has placed it in our very blood), I should consider a law which would *oblige* men to live or die by the recipes of some of my medical brethren, though they be legally qualified men, as similar on the part of the government to licens-

* Dictionnaire Infernal, art. "Girtanner," p. 253.

ing a system of slow poisoning. I repeat we are obliged to work in the dark in medicine, and that Morison's pills may succeed when most other medicines have failed, or Morison's pills may fail when other modes of treatment may succeed. Under such circumstances, *at least let us do as little harm as possible, and not pretend to teach Nature, by putting metals into the body which she has not put there herself.*

I shall now return again to Sir J. Sinclair's work, since I observe he states briefly that he considers Cornaro's plan of life will never be generally followed, on account of its requiring so much self-denial. As there is something in this observation, certainly, I shall here say a few words on what appears the best mode of counteracting the bad effects of excess in eating or drinking.

Since the dinner-meal is almost universally a *much* heavier meal than Cornaro's, it may sometimes be necessary to adopt the French plan of taking a small cup of *café noir* afterwards, or better, a few glasses of a *stronger* wine than Cornaro took (such as sherry, for example), *taking care also to use no mental or bodily exercise for at least two hours afterwards*; but *after* this period has elapsed, exercise will be additionally desirable to *complete* digestion. Mr. Abernethy is stated to have slept daily immediately after dinner; most animals do so; and in Sir B. Harwood's experiment, the food of the dog that was made to run immediately after a full meal was found undigested, while that of the one fed at the same time and in similar quantity, and allowed to sleep, was completely assimilated. But, assuredly, if we eat more

than Cornaro, though we should more particularly avoid exercise *just afterwards*; we should, *on the whole*, take a proportionately greater quantity of exercise than he did. This is necessary to prevent the surplus remaining in the system, and producing disease. Additional work is put on the kidneys in this case, and the urine passed three or four hours after a *full* meal is generally turbid; a sign that we have eaten too much, and that we should carry off such excess now by a walk. If this does not succeed, and flatulence, furred tongue, &c. &c., remain as habitual symptoms, our medicine for indigestion (p. 75) may be commenced, *weak* brandy and water, without sugar, being used as drink for dinner; but an adequate amount of exercise *at proper times* will generally render this unnecessary. Sir J. Sinclair met with an old woman above eighty who attributed her long life to a walk of a mile or two, which she always took every day, an opinion in which she was most probably right.

As an increase of exercise is the remedy I would suggest for excess of eating (for we all take more than nature requires), so it is also the only one for excess in the use of alcoholic liquors. *Sweat* these out by *strong* exercise, if moderate does not succeed.

APPENDIX.

On Mesmerism — Author's Successful Attempt at Partial Self-Mesmerisation, with Theory — Author's Experiment on the Effect of Gelatinous Silica on a Cat — Experiments on the Antiseptic Powers of Camphor and Carbonat of Soda, &c. &c.

HAVING, at page 19 of this work, expressed my belief in mesmerism (at all events, to *a certain extent*), I shall here state some of my reasons for such belief, and commence with some experiments made on myself, certainly *to me* the most satisfactory evidence of truth in such a matter, at least, while consciousness remains. I believe they entitle me to the discovery, such as it is, of the possibility of partial *self-mesmerisation*; and if it be possible for some persons to mesmerise themselves, this is a great step towards proving that mesmerism depends rather on a *peculiar sensation* produced in the individual, than any actual communication or abstraction of a certain supposed fluid. Fainting obviously depends on a *sensation*, though of a different kind, for a person may, of course, faint without loss of blood.

Let any one close his eyes, and then let another

person or himself draw the inside of the middle or fore finger *very lightly* downwards over the upper eyelid, for many times in succession, and it will in some cases (for the effect is not, perhaps, even general), be found impossible to open the eyes till a current of air is directed on them, or till the eyelashes or under eyelid are touched. The experiment should be made five or six days in succession for a quarter of an hour each day, to give it a fair trial.

I succeeded on a second or third trial in making the above experiment *on myself* some time ago, after having often had mesmerism tried on me in vain to produce any further effect. A person mesmerised me *thus far* lately at Brussels, by passes slightly touching the points of hair on the head and over the face *without having touched the eyelids or eyelashes at all*.

We may infer from this, that the loss of power of opening the eyes is dependent not on any fluid or influence directly transmitted to the eyelids, *but to an influence on the body generally*, which is *afterwards* more especially directed to the eyelids by the vital principle, and causes the loss of power in question. This inability to open the eyes is well known as an incipient mesmeric effect. I have known a case of a person who, by her own confession, never could be more mesmerised than this, though she had been experimented on by myself and others a great number of times.

I found when the mesmeriser had thus destroyed my power to open my eyes, that he restored it immediately on two occasions by blowing, at my request, against a thick sheet of brown paper placed over them without touching, and sufficiently large to cover my face and a great part of my body. As I did not perceive any motion in the external air *next my face*, I think I can only have derived the power of opening my eyes from

some influence that passed directly *without air, through the paper itself*.

Second Experiment.—Repeated this last experiment, putting a thick *pasteboard*-box over the face, and paying still more attention to the air. A breath against this box, TOO SLIGHT FOR ME TO PERCEIVE NOW CERTAINLY, enabled me to open my eyes directly. The box was a little larger than the face, and deep enough to cover both ears without touching any part.

Third Experiment.—Mesmerisation by the same individual. No effect, except a slight weight on the eyelids, but not sufficient to prevent my opening them myself.

Fourth Experiment.—Mesmerised in the evening after dinner, feeling at the time very sleepy. It is singular that no more effect was produced this time than the last. Indeed, the mesmeric manipulations rather tended to keep off ordinary sleep, contrary to what I had expected. This, however, tends to confirm the supposition that the mesmeric is totally different from common sleep, as was to be presumed.

Fifth Experiment.—No effect.

Sixth Experiment.—No effect, though continued, perhaps, longer than any of the previous experiments.

The above experiments, with one exception, were made on *successive* days. In the case alluded to, there was only the interval of one day; and it is curious to observe that my susceptibility to mesmerism seemed to *diminish* by repetition, contrary to the general rule. It, however, probably, did not diminish. The reason of such *apparent* diminution I conceive to have arisen from my being in a better state of health after the second mesmerisation, whereby the operator's power was diminished, for the experiments were continued the same length of time, or nearly so, in each case, viz. about half-an-hour.

It was in consequence of having, about two years ago, made similar experiments to the above *first and second* experiments, that the theory of hearing, itself depending on the vibration of an elastic *imponderable* medium, and not on the air, suggested itself to me; and my review of the arguments for and against this theory will be found at length in the "Mechanics' Magazine" for that period.

1st, That there is a spiritual power within us which perceives far more quickly than we can, and, 2dly, also on some occasions perceives effects which we cannot perceive at all, I consider to be demonstrated by numerous points in physiology. Without going into details, our being *obliged instantly* to close our eyes when any one suddenly thrusts his hand forward to them, is a proof of the first position; for here *an instinctive spiritual power* closes our eyes for us BEFORE our will has time to operate. And as a proof of the second position may be instanced the movements of the fingers of a *thoroughly practised* musician, and, perhaps, also the vomiting or purging which generally follows the introduction of any substances injurious to our constitution into the stomach. [The reader who wishes to see similar metaphysical points discussed at greater length, is referred to the work "Thoughts," published by Simpkin and Marshall in 1839.]

Now to apply these facts to our *first and second* mesmeric experiments, it is pretty clear that such spiritual power may perceive a motion in the air which we cannot perceive. Although, therefore, *I*, in such experiments, could perceive no motion, *it* might; and of course I could not say there was no motion, for to have been *certain* of this my face should have been enclosed in an *air-tight* box, which is, perhaps, next to impossible. Consequently, I cannot pretend to say that the cause of my regaining the power to open my eyes did not depend

on this presumed spiritual power being excited by a movement in the air *too slight for me to perceive*. If this conclusion be not the right one, then we seem *necessarily* forced into the belief of the doctrine of a mesmeric medium, subtile enough to pass instantaneously through thick paper or pasteboard, which is generally agitated by any movement in the air, *but which may be agitated and made to pass through paper and pasteboard, leaving, of course, the air that generally accompanies it, behind*. On this view of the subject, the demesmerising power of a current of air, which Mr. Braid, in his work (*On Hypnotism*, p. 30), confesses he could not account for, depends on such motion in the air, *causing a motion in an imponderable medium*.

It follows that the demonstration of such a medium is not more than probable from the above experiments, since I could not be *quite certain* that no motion in the air *about my face* took place. Mesmerisation and demesmerisation by the *will alone, without any motion in the air*, must be considered the sole best evidence of the existence of such a medium. The Rev. H. Townshend and others state they have succeeded in mesmerising by the will alone, the patient being sometimes half-a-mile or so off; but, in this case, it is also necessary to be sure the patient was not aware of the mesmeriser's intentions, for in two cases of this sort which I examined, I traced the whole of the *supposed* mesmerisation to this knowledge on the part of the patient. When I arranged the experiment so that *they could not know when it was to begin*, it failed perfectly. Without, therefore, presuming to say that Mr. Townshend was deceived, or that mesmerisation by the will *alone* is impossible, I certainly shall not believe in it without the satisfactory evidence of personal experience. The present reflections, however, seem to speak to a certain extent in its favour,

since they display clearly the fact of a spiritual power distinct from ourselves, *being able to perceive what we cannot, even with all our best attention, perceive*. If it be possible for a volition of the human mind, then, to put this medium in motion, as Townshend thinks, there is no difficulty to believe that a spiritual power within us may perceive this motion, *though we cannot perceive it*, and consequently be so affected as to cause the mesmeric state in a very susceptible individual. Mark, then, in mesmerising by the will alone at great distances you act *first*, necessarily, on a spiritual power in the person distinct (so to speak) from self, and such power has been proved to be far more susceptible of impressions than we are. It is only a question of distance, then; and why not be susceptible at great distances if the cause be strong? I want but *facts* to believe.

I have had an opportunity while this work has been going through the press to try the experiment with *gelatinous* silica (suggested p. 22) on a cat. I commenced with half a teaspoonful well mixed with milk, and finding the animal not at all injured by this quantity, I repeated it three or four times, till at last the quantity given amounted to two or three teaspoonsful, the animal still taking it freely, and being uninjured by it.

In order to see if the silica passed off with the fæces, I put these into a crucible and kept them at a red heat for two or three hours, until all combustible matter was destroyed. The powder remaining behind was of a greyish white, and I should say amounted to one-fifth or one-sixth of the *bulk* of the fæces incinerated. Muriatic acid being poured upon them, no effervescence took place; nor by long digestion, either in this or nitric acid, was there scarcely any solution effected, whether diluted with water or not. It hence consisted almost entirely

of dry silica powder. I may observe that the silica appeared to have acted like chalk in increasing the consistency of the fæces, which were also of a lighter colour than usual. They were passed about *twenty-eight hours* after the animal had taken the *last* two teaspoonsful of the gelatinous silica, and consequently ample time had been allowed to the digestive and assimilative powers to act upon and decompose it, if possible. Some fæces passed by the animal about three hours after the last were much more watery and darker, and contained silica, though in less proportion than the last. The cat, I am glad to say, has remained quite well now three weeks after the experiment.

Subsequent authors appear to have been unable to repeat successfully Dr. Brown's experiment of the conversion of silica into carbon, and the present experiment would seem likewise to support Smith, Brett, and others, as to the impossibility of succeeding; for if the vital powers cannot effect such transformation (and the present experiment supports the opinion they probably cannot), it is not very reasonable to expect it can be effected in the chemist's laboratory.

I should have stated at the end of the Essay on Cornaro (p. 24) that both phosphorus and phosphoric acid have been tried on the human subject, and that the former acts on the generative organs something like cantharides, and that the latter (far less dangerous) appears to act as a tonic, like dilute sulphuric acid. Phosphorus should never, of course, be used but in homœopathic doses—if even then. As regards Fluorine, it is *very* doubtful if it has even yet been isolated.

I have lately made experiments on the antiseptic power of strong camphor-water, and though it seems to

have such power, it is inferior to salt-water, or solution of carbonat of soda, in preserving animal substances from putrefaction; as regards *milk*, in two trials it failed altogether. In my Essay "On the Anti-Inflammable Powers of Carbonat of Soda and other Salts," published in the Philosophical Magazine for 1839, I have given many experiments to shew that carbonat of soda preserves both animal and vegetable substances from putrefaction; and I may here add that I have since found carbonat of soda to take away the bad smell from meat, even *after it has begun* to decompose. Such being facts, I cannot doubt that one object of the soda in the blood and body generally, is to aid the vital power in preventing decomposition, since, in the paper above alluded to, it is shewn that milk was preserved from mouldiness, and kept free from a bad smell, for three weeks, by solution of carbonat of soda alone. I do not by this mean to assert that such antiseptic power is confined to carbonat of soda; on the other hand, there can be little or no doubt but that *all* the other *saline* matters in the body act equally as antiseptics, the common salt it contains having been known to act as such on dead matters from time immemorial.

The above considerations lead me to put even additional stress on the passage marked in Italics at p. 34 of my introduction to Raspail's Essay, since camphor-water is obviously not so powerful an antiseptic as saline solutions. I, therefore, here again may recommend carbonat of soda—not indeed, perhaps, as a vermifuge, but what is better—as a *preventive* of worms, acting along with other salines in *really* purifying the blood by tending to keep it *red, fluid*, and free from even the slightest symptom of *putrefaction*. The quantity of this salt that nature has put in the bile shews at once how essential it is for healthy assimilation and digestion. I repeat, then,

until worms have appeared, let us use more soda, common salt, &c., than Raspail recommends. When they have appeared it is time to insist more on camphor, aloes, and vegetable anthelmintics generally.

I have stated in the note, p. 41, that Mr. Cross's acari propagate by division. Being anxious to ascertain whether they propagated by ova also, as Raspail says the acari of cheese do, I lately wrote to Mr. Weekes to forward me the extract from my letter to him on that subject in 1843, made on acari he sent me enclosed in a glass tube. He has kindly done so, and I find by these observations, made with much care, and written while *still fresh in memory*, that these acari seemed to propagate *both* by division and eggs; but certainly by eggs, or *vivaparously*:—

“The three remaining acari Crossi gradually enlarged in size, their bodies becoming *almost divided into two*; and shortly afterwards eight or ten little ones were found moving over the tube. Now all are dead. These little ones might have burst ova, as you say; but I did not see them. They were scarcely visible except for their motion.”

I may here state that Mr. Noad has not been able to repeat the experiments of the production of Mr. Cross's acari in *closed* atmospheres, although he tried during two years; and that having lately myself much observed the mites which live in *dry* cheese, they appear *very* similar to the acari in question, though without seeing both *side by side*, I could not pronounce on their identity. For the present, therefore, I deem it the wisest plan to have no opinion on this difficult subject.

Having just received from Mr. Noad himself an account of his experiments, I shall conclude this work by stating, 1st, that Mr. Noad's apparatus consisted solely

of glass and iron ; and, 2dly, that although no acari ever appeared *within* the receivers, yet “ hundreds made their appearance, after about six months, on and about the *terminal* cells of the batteries, of which *the circuit was kept closed during the whole time* ; but not in those batteries where it was left incomplete, though several were tried in the same room as the former.”

It would appear to follow from this extract, that the nature of these acari is to congregate *in the vicinity of an electrical current*,—perhaps, in consequence of the *decompositions* there going on ; much, in fact, on the same principle as Raspail mentions their place of abode generally to be in *old* furniture, ill-conditioned ulcers, and where-soever the *fermentation caséique* (as in old cheese) is present. Taken which way we will, their appearance only about a *closed* electrical circuit is a curious fact. Where decomposition is going on, they certainly have more *chance* of finding nourishment.

Against all this Mr. Weekes writes me that Mr. Cross has lately succeeded in *closed* atmospheres.

THE END.



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